

PRIVATE
DEPOSITORY

MONTHLY NOTICES
OF THE
ROYAL ASTRONOMICAL SOCIETY.

VOL. LIV. No. 2. DECEMBER 1893.

Fellows of the Society are informed that post-cards containing the titles of papers to be read at the evening meetings will be sent to those who shall express their wish to receive them. Fellows desiring to receive the post-cards are requested to send their names to the Assistant-Secretary, Mr. W. H. Wesley, Burlington House, London, W.

In order to enable the Secretaries to prepare complete lists in time for the issue of the post-cards, authors are particularly requested to send in their papers not later than the Tuesday preceding the day of meeting.

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VOL. LIV.

DECEMBER 8, 1893.

No. 2

Captain W. de W. ABNEY, R.E., C.B., D.C.L., F.R.S., President,
in the Chair.

Captain Edmond Herbert Hills, R.E., Darland House,
Chatham;

Charles Herbert Edmund Rea, 223 Norwood Road, Herne
Hill, S.E.; and

William Shackleton, Royal College of Science, South Ken-
sington, S.W.,

were balloted for and duly elected Fellows of the Society.

The following candidate was proposed for election as a Fellow
of the Society :—

William Ford Stanley, F.G.S., F.R. Met. Soc., Cumberlow,
South Norwood, S.E. (proposed by T. Cushing).

Seventy-five presents were announced as having been received
since the last meeting, including, amongst others :—

A. Auwers, *Tafeln zur Reduction der Sternörter*; A. Cayley,
Collected Mathematical Papers, vol. vi.; R. A. Gregory, *The
Vault of Heaven*; W. Schur, *Untersuchungen über den Verlauf
der systematischen Correctionen*; F. Terby, various papers on
Jupiter and *Saturn*, presented by the Authors; A. Steinhausen,
Logarithmen-Tafeln, and F. von Mocknik, *Logarithmen-Tafeln*,
presented by General Tennant; C. Easton, *La Voie Lactée*, pre-
sented by Professor Bakhuyzen.

On the Cause of the per saltum Change in the Errors of Hansen's Lunar Tables as usually compared with Observation, being an Appendix to a paper in the "Monthly Notices," vol. liv. No. 1, which exhibits these errors from 1847 to 1892. By E. J. Stone, M.A., F.R.S., Radcliffe Observer.

I have recently received several inquiries on points connected with the explanation which I have given of the *per saltum* change, which certainly took place about 1864, between the tabular places given by Hansen's Lunar Tables and the corresponding observations of the Moon, when compared in the usual manner. These inquiries show that I have not yet succeeded in making my views clearly understood. I have thought it, therefore, desirable to draw up the following statement of my views, which I hope may render them at least intelligible to mathematical astronomers.

But before proceeding with these explanations, it is perhaps desirable to first call attention to the fact that if $P \sin (at + \beta)$ denotes a long inequality, neither the omission of a real inequality of this form from the tabular expression of the Moon's longitude nor the erroneous introduction of such an inequality can introduce *per saltum* changes in the errors of the Tables. It will render the determinations of the values of the "epoch correction" and "mean motion" from a discussion of residuals erroneous; and it will lead, therefore, at some time or other, to progressive changes in the tabular errors for many successive years, but not to such changes in the errors of the Tables as those shown on page 10 of the *Monthly Notices*, vol. liv. No. 1.

The errors discussed in the present paper will be found to arise from the use of two independent systems of time-measures, whilst the ratio of the units is only approximately known.

In the comparisons between the theoretical and observational results of astronomy for the determination of the numerical values of the data involved, it is necessary to fix the positions of the meridian of the observer in terms of the variable, t , which is employed to fix the geocentric positions of the centres of gravity. And it becomes necessary for my purpose to discuss the accuracy with which this is effected by the methods adopted in practical astronomical work.

The "mean tropical year" referred to in my papers is that of practical astronomy; an interval of time in terms of which motions can be, and are, measured; and not any fictitious or imaginary interval of time which could not be employed for such purposes.

At the equinoxes the Sun's centre passes through the plane of the Equator, and its north polar distance is 90° .

The times by the observatory clocks of these equinoctial passages can either be directly observed at different stations on the Earth's surface, or deduced from the observations usually made. The recorded times will be subject, as usual, to errors of observation and reduction; but these errors can be confined within small limits, and will not be accumulative with the time. The intervals between successive transits of the Sun through the same equinox are the "tropical years" with which we are concerned.

The lengths of the "tropical years" can, therefore, be determined directly from observation expressed in terms of the mean sidereal day, hours, minutes, and seconds, provided the errors of the observatory clocks on the local sidereal times can be correctly determined.

The lengths of the tropical years can be corrected for secular and periodic variations from the results of mathematical investigations, in which the mean tropical year itself, or some definite part of that interval of time, is taken as the unit; and the length of the mean tropical year at the selected epoch will thus become determinable in terms of mean sidereal seconds, subject to the condition already mentioned. The interval of time which can thus be deduced from observation expressed in mean sidereal seconds is the mean tropical year with which we are concerned in practical astronomical work.

The ratio of the mean tropical year to a mean sidereal day will be absolutely independent of any particular fraction of a mean tropical year, which we may, as a matter of convenience, adopt for the "unit day"; and its numerical value can be approximated to without limit provided only the local sidereal times can be accurately deduced from observation.

The following is a general statement of the course followed to render the local sidereal times determinable from observation. It will be found that the invariability of the ratio of the mean tropical year to the mean sidereal day is a fundamental condition of the success of the methods adopted for the determination of the local sidereal times; and that no approximative value of this ratio is used in any part of the work, except in terms so small that any errors which may arise from its use will be confined within very small limits; and that such errors, if sensible, are finally eliminated from the determinations of the local sidereal times, and prevented from accumulation.

The tabular right ascensions of the clock stars are brought up from epoch in terms of the mean tropical year and some definite sub-multiple,

$$\frac{N}{2\pi} \text{ (of a mean tropical year),}$$

where N is the circular measure of some assigned angle, as convertible units of time. The fractional part of the mean tropical year thus employed as a unit in stellar astronomy is treated as the "mean solar day." These are mere statements of facts.

The local sidereal times are deduced by the aid of clocks, whose rates are steady over intervals of a sidereal day, from comparisons between the recorded clock times at the transits of the different clock-stars over the meridian and their tabular right ascensions; and, if any sensible errors exist in these local sidereal times, they are determined, and when sensible duly allowed for in practice, from direct comparisons between the local sidereal times at the transits of the Sun's centre over the meridian and the corresponding right ascensions of the Sun's centre as deduced from the observed north polar distances of the Sun and the obliquity of the ecliptic.

The result of such a process must be to entirely prevent the accumulation of error on the local sidereal times, as found from observation, due either to errors in the adopted tabular right ascensions of the clock stars at epoch; the use of slightly erroneous values of the precession constant, and of proper motions of the stars; and of any small errors due to imperfect allowances for the changes of the right ascensions of the clock-stars between the times for which they are directly computed and the times of meridian passage. The local sidereal times can thus be correctly found from observation, and they will strictly correspond to definite "mean solar times" on the scale of time-measurement adopted in stellar astronomy.

We are placed in a position to approximate without limit, by the process described, to the ratio of the "mean tropical year at epoch" to the "mean sidereal day at epoch."

If we denote the exact ratio of the "mean tropical year" to the "mean sidereal day" by

$$\frac{2\pi}{N_0} + 1,$$

where N_0 is the circular measure of an angle to whose value we are continually approximating; and, if Ω denotes the mean motion in right ascension of an observer's meridian in the "unit day," equal to

$$\frac{N}{2\pi} \cdot (\text{mean tropical year}),$$

which is adopted in stellar astronomy, we shall have

$$\Omega = 2\pi \frac{N}{N_0} + N.$$

If we knew and adopted the accurate value of N_0 to fix the proportionate part of the mean tropical year which is used as the "unit day" in stellar astronomy, we should then have

$$\Omega_0 = 2\pi + N_0,$$

and the "unit day" would, in this case, be the interval of time which astronomers have wished to adopt as their fundamental "unit day," an interval of time which, for distinction between the different "unit days" which are adopted in practice, I shall call "a physical mean solar day."

The "physical mean solar day" is the interval between consecutive transits of the meridian over the Sun's centre when cleared from secular and periodic variations, which in this case are as large proportionately to the interval of time measured as in the corresponding case of the tropical years already mentioned.

But if we adopt a definite angle N to fix the "unit day," the expression for the right ascension of the observer's meridian, A , as a function of the time, t , on the scale under consideration, assumes the form

$$2\pi\iota + A = A_0 + \left(2\pi \frac{N}{N_0} + N\right)t + \sigma t^2 + \text{nutation in R.A.},$$

where ι is an integer, A_0 and N_0 angles whose values have to be found from a discussion of these equations when formed with corresponding values of A and t .

The *exact values* of the angles

$$A_0 \text{ and } \Omega = 2\pi \frac{N}{N_0} + N$$

are unknown with the system of time measures, t , which is used in practical stellar astronomy, and which is fundamentally based upon the Sun's motion and the mean tropical year.

But in finding the local sidereal times from observation, if A_r denotes the right ascension of the observer's meridian at the time $t=r$, and $r+\delta r$ the required time when the right ascension of the meridian is A , we shall have

$$A_r = A_0 + \left(2\pi \frac{N}{N_0} + N\right)r + \sigma r^2 + \text{nut.} - 2\pi\iota$$

$$A = A_r + \left(2\pi \frac{N}{N_0} + N\right)\delta r + \sigma (2r\delta r + \overline{\delta r}^2) + \frac{\delta \cdot \text{nut.}}{\delta t} \cdot \delta r.$$

The small terms

$$\sigma (2r\delta r + \overline{\delta r}^2) + \frac{\delta \cdot \text{nut.}}{\delta t} \cdot \delta r$$

can be applied, if it is thought necessary, with an approximate value of δr , but they are so small that they can, in practice, be generally neglected; and we have

$$\delta r = \frac{A - A_r}{2\pi \frac{N}{N_0} + N}.$$

If we adopt for N the nearest approximative value to N_0 available at the time we shall have

$$\frac{N}{N_0} = 1 + x,$$

where x will be generally some small fraction, but may be zero; and if we adopt for A_0 , as an approximative value, l , we shall have

$$A_r = l + Nr + \sigma r^2 + \text{nutation} - 2\pi(t-r) + \delta l + 2\pi x r;$$

and, therefore,

$$t = r + \frac{A - (l + Nr + \sigma r^2 + \text{nutation} - 2\pi(t-r)) - \delta l - 2\pi x r}{2\pi + N + 2\pi \cdot x};$$

or if

$$\begin{aligned} A'_r &= l + Nr + \sigma r^2 + \text{nutation} - 2\pi(t-r) \\ t &= r + \frac{A - A'_r - \delta l - 2\pi x r}{2\pi + N + 2\pi x}. \end{aligned}$$

But if

$$t = r + \delta r$$

is the time which corresponds to the transit of a star, the centre of the Sun, or of the Moon, &c., over the meridian of the observer; A , the right ascension of the meridian at the time of transit, and, therefore, the right ascension of the object observed at the time

$$t = r + \delta r;$$

$\phi(t)$ the function which gives the tabular right ascension for the time; and $\delta\phi$ the true error of the tables in right ascension; we shall have

$$A = \phi(r) + \phi' \cdot \delta r + \phi'' \cdot \frac{\delta r^2}{1 \cdot 2} + \&c. + \delta \cdot \phi$$

where the expansion has to be carried to the required degree of approximation. We can, of course, compute the values of $\phi(t)$ for each hour

$$\phi\left(r + \frac{1}{24}\right), \phi\left(r + \frac{2}{24}\right) \&c.$$

and then determine

$$\phi\left(r + \frac{h}{24} + \delta t\right)$$

by expansion. We have, therefore,

$$A = \phi(r) + \phi' \cdot \frac{A - A'_r - \delta l - 2\pi x r}{2\pi + N + 2\pi x} + \&c. + \delta \cdot \phi.$$

And if, in finding the tabular right ascensions at the meridian transits for comparison with the true right ascensions, we neglect the effects of the small terms δl , $2\pi x$, we necessarily include in the differences between the observed right ascensions

and the "tabular right ascensions" the effects of the neglected terms δl and $2\pi v$ amongst those due to the errors of the Tables, $\delta\phi$; and similar remarks, of course, apply to the formation of the residual errors in N.P.D.

The importance of the errors introduced from neglecting the terms δl and $2\pi v$ will depend upon the magnitudes of the factors $\phi'(r)$, &c. In the case of the stars they will be practically insensible. But if, as is usual, we neglect the effect of these small terms in finding the tabular right ascensions $\alpha_1, \alpha_2, \alpha_3, \dots$ at the transits of the different clock-stars over the meridian of an observer on a given "sidereal day," we shall have the following exact equations of condition.

$$(1) \quad A_r + \left(2\pi \frac{N}{N_0} + N\right) \delta r_1 = \phi_1(r) + \phi_1' \cdot \delta r_1 + \&c. + \delta \cdot \phi_1 = \alpha_1 + \delta \alpha_1$$

$$(2) \quad A_r + \left(2\pi \frac{N}{N_0} + N\right) \delta r_2 = \phi_2(r) + \phi_2' \cdot \delta r_2 + \&c. + \delta \cdot \phi_2 = \alpha_2 + \delta \alpha_2$$

where $\delta \alpha_1, \delta \alpha_2, \delta \alpha_3$ will denote the total corrections which the tabular right ascensions of the clock-stars require, including any which may be due to the neglect of the small terms δl and $2\pi v$. But if c_1, c_2, c_3 are the times in seconds indicated by the observatory clock at the meridian transits of the clock-stars; if we denote by c_0 the *unknown clock* time which accurately corresponds to the time $t = r$, on the scale of time under consideration; and if the "unit day" which equals the $\frac{N}{2\pi}$ -th part of a mean tropical year is put equal to 86400 seconds, we shall have

$$\delta r_1 = \frac{\rho (c_1 - c_0)}{86400}, \quad \delta r_2 = \frac{\rho (c_2 - c_0)}{86400}, \quad \&c.,$$

where ρ will be some number which will remain constant so long as the clock has any steady rate, and the same "unit day" is adopted and put equal to 86400 seconds. We shall have, therefore,

$$(3) \quad A_r + \frac{2\pi}{86400} \left(\frac{N}{N_0} + \frac{N}{2\pi} \right) \cdot \rho (c_1 - c_0) = \alpha_1 + \delta \alpha_1$$

$$(4) \quad A_r + \frac{2\pi}{86400} \left(\frac{N}{N_0} + \frac{N}{2\pi} \right) \cdot \rho (c_2 - c_0) = \alpha_2 + \delta \alpha_2$$

&c. = &c.

The angle N_0 which appears in these equations is a constant, although its exact value is unknown, and N is the value assigned to the angle which is actually used in practice to fix the length of the "unit day."

If, therefore,

$$\rho \left(\frac{N}{N_0} + \frac{N}{2\pi} \right) = R,$$

R will be a constant, so long as ρ is a constant or the rate of the clock used is steady, and the same "unit day" is adopted. We shall have, therefore,

$$(5) \quad \frac{A_r}{2\pi} \cdot 86400 + R(c_1 - c_0) = \frac{\alpha_1}{2\pi} \cdot 86400 + \frac{\delta\alpha_1}{2\pi} \cdot 86400$$

&c. = &c.

where both sides of the equations are expressed in terms of the "mean solar seconds" of the "unit day," and from these equations values of the constants

$$\left(\frac{A_r}{2\pi} \cdot 86400 - Rc_0 \right)$$

and of R could be found. But as we require the clock errors on local sidereal time we proceed otherwise. The equivalent of the "unit day," or 86400 mean solar seconds, on the scale under consideration, in sidereal seconds is

$$86400 \left(\frac{N}{N_0} + \frac{N}{2\pi} \right)$$

and therefore replacing the 86400 mean solar seconds by its equivalent in the equations (3); (4); &c., we have

$$(6) \quad A_r + \frac{2\pi}{86400} \cdot \rho(c_1 - c_0) = \alpha_1 + \delta\alpha_1$$

$$(7) \quad A_r + \frac{2\pi}{86400} \cdot \rho(c_2 - c_0) = \alpha_2 + \delta\alpha_2$$

$$\text{or } (8) \quad \frac{A_r}{2\pi} \cdot 86400 - \rho c_0 + \rho c_1 = \frac{\alpha_1}{2\pi} \cdot 86400 + \frac{\delta\alpha_1}{2\pi} \cdot 86400$$

$$\&c. = \&c.$$

where both sides of the equation are now expressed in terms of mean sidereal seconds.

The "rates" R and ρ and the corrections

$$\frac{A_r}{2\pi} \cdot 86400 - Rc_0$$

$$\frac{A_r}{2\pi} \cdot 86400 - \rho c_0$$

are different in the two cases, and the values which would be found for them would necessarily differ when deduced from observation. The adjustments for such changes of scale are necessarily included in the determination of clock errors and rates when accurately found from observation.

Denoting

$$\frac{A_r}{2\pi} \cdot 86400 \text{ by } A_r^s$$

$$\frac{\alpha_1}{2\pi} \cdot 86400 \text{ by } \alpha_1^s, \&c.,$$

we have

$$(9) \quad A_r^s - \rho \cdot c_0 + \rho \cdot c_1 = \alpha_1^s + \delta \alpha_1^s$$

$$(10) \quad A_r^s - \rho \cdot c_0 + \rho \cdot c_2 = \alpha_2^s + \delta \alpha_2^s$$

$$\&c. = \&c.,$$

where both sides of the equations are now expressed in mean sidereal seconds.

When the equations are thus expressed in sidereal seconds we can reduce them to $0^h 0^m 0^s$ sidereal for comparison with each other and with the results found from the observations made on a consecutive "day," and thus facilitate the determination of the rate constant ρ .

If we put $\tau^s + \delta \tau^s$ for the unknown difference of time

$$A_r^s - \rho c_0,$$

which appears in all these equations, we shall have

$$(11) \quad \tau^s + \rho c_1 + \delta \tau^s = \alpha_1^s + \delta \alpha_1^s$$

$$(12) \quad \tau^s + \rho c_2 + \delta \tau^s = \alpha_2^s + \delta \alpha_2^s,$$

$$\&c. = \&c.$$

And, if we provisionally neglect the small errors $\delta \alpha_1^s$, $\delta \alpha_2^s$, and find definite values of τ^s and ρ , from the equations

$$(13) \quad \tau^s + \rho c_1 = \alpha_1^s$$

$$(14) \quad \tau^s + \rho c_2 = \alpha_2^s, \&c.,$$

the observatory clock when corrected by the "errors" τ^s and ρ will give the "local sidereal times" as directly deduced from observation.

It will not, however, be possible to deduce the right ascension, A , of the observer's meridian accurately from the local sidereal times as thus found from the observed clock-time c by the formula

$$(15) \quad A = \left(\frac{\tau^s + \rho c}{86400} \right) 2\pi$$

unless the effects of the errors $\delta \alpha_1^s$, $\delta \alpha_2^s$, which have been neglected in finding τ^s and ρ , are insensible.

But if $c_\odot$ is the recorded time of the Sun's transit by the clock on the "sidereal day" under consideration, and we put the Sun's right ascension, α ,

$$= \frac{\tau^s + \delta \tau^s + \rho c_\odot}{86400} \cdot 2\pi,$$

the correction $\delta \tau^s$, which the local "sidereal time" requires when found as described, can be deduced from this equation if the Sun's right ascension can be otherwise found from observation.

But if the north polar distances of the Sun's centre are

observed, the right ascensions can be found in terms of the observed north polar distances, and of the obliquity of the ecliptic. When the observations are made near the equinoxes, any small errors in the adopted value of the obliquity will not sensibly affect the resulting values of the right ascensions of the Sun thus deduced, and a mean value of $\delta\tau^s$ can thus be found. And by extending the discussion to observations of the Sun made over a year, the mean correction, $\delta\tau^s$, required by the local sidereal times found from the tabular right ascensions of the clock stars, and any correction required by the adopted value of the obliquity of the ecliptic, can be deduced from a general discussion of the equations of condition,

$$(16) \dots\dots\dots \frac{\alpha}{2\pi} \cdot 86400 = \tau^s + \delta\tau^s + \rho c_{\odot},$$

where α is the R.A. of the Sun's centre, as deduced from the observed N.P.D., and corrected obliquity. This is the method which is practically followed by astronomers. If the corrections $\delta\tau^s$ thus found were sufficiently large to render such a step necessary, they would have to be applied to the "local sidereal times" at once. But in the present state of astronomy the mean errors, $\delta\tau$, are so small that it is found sufficient to apply them every few years to the right ascensions of the clock-stars found from the local sidereal times

$$\tau^s + \rho c_{11}, \tau^s + \rho c_{21}, \&c.$$

In this way more accurate tabular right ascensions of the clock-stars are continually deduced and adopted for future use; and any accumulation of the effects of errors of the tabular right ascensions of the stars on the local sidereal times is thus rendered impossible.

The time, t , which, on this scale, will correspond to a given local sidereal time A^s will therefore be

$$\begin{aligned} (17) \dots\dots\dots t &= r + \frac{\rho(c - c_0)}{86400 \cdot \text{unit seconds}} \\ &= r + \frac{\rho(c - c_0)}{86400 \left(\frac{N}{N_0} + \frac{N}{2\pi} \right) \text{sidereal seconds}} \\ &= r + \frac{(\rho c + \tau + \delta\tau) - (\rho c_0 + \tau + \delta\tau)}{86400 \left(\frac{N}{N_0} + \frac{N}{2\pi} \right)} \\ &= r + \frac{A^s - A_r^s}{86400 \left(\frac{N}{N_0} + \frac{N}{2\pi} \right)} \end{aligned}$$

where the seconds are mean sidereal seconds.

The use of an erroneous value, $A_r^{s'}$ for A_r^s , will necessarily

throw upon the determinations of the time, t , which correspond to a given local sidereal time A^s , any errors made. In fact, if we employ

$$A_r^{s'} = \left[\frac{l + Nr + \sigma r^2 - 2\pi(1-r)}{2\pi} \right] \cdot 86400^s$$

instead of

$$A_r^s = A_r^{s'} + \frac{\left(\delta l + 2\pi r \frac{N - N_0}{N_0} \right)}{2\pi} \cdot 86400^s = A_r^{s'} + \delta l^s + 86400^s \cdot x r$$

we cannot escape throwing the errors made by neglecting

$$\delta l \text{ and } x = \frac{N - N_0}{N_0}$$

upon the determinations of the time, and therefore upon the epoch corrections and mean motions which are deduced from residual errors thus erroneously formed.

The exact errors of the times, t , when thus determined from the "local sidereal times" by neglecting δl and x , cannot be assigned so long as δl and x are unknown, but the differences between the errors made when using different constants, l' and N' , instead of l and N , in the expression of the Sun's longitude can be computed with very great accuracy if the differences between the angles N , N' , and N_0 are small. And the times, t , on the required scale can always be found from accurate observed geocentric positions of the Sun subject only to any errors which may arise from imperfect allowances for the small secular and periodic terms in the theoretical expressions of the Sun's co-ordinates.

The values of $A_r^{s'}$ are the "sidereal times at mean noon" for the standard meridian given in the Nautical Almanacs. If the "mean noons" are defined as the instants when the local sidereal time on a given sidereal day is equal to $A_r^{s'}$, the results are arithmetically correct. But if the usual assumption is made that $t = r$ (the value for which $A_r^{s'}$ is computed) when the scale of time-measurement is that which is generally adopted in practical astronomical work, the assumption will not be true unless l and N happen to have assigned to them certain definite values, l_0 and N_0 ; and this can only be true for one pair of such values at a given epoch. In other cases an unknown time, τ , must be introduced to transfer the "epoch" to a "mean noon" as defined.

The conclusions at which I have arrived respecting the errors made in referring the tabular places to the observer's meridian and in finding the mean solar times, t , which correspond to a given local sidereal time, A^s , follow as necessary consequences of the fact that the system of time-measures, t , in stellar astronomy is based fundamentally upon the motion of the Sun by assigning definite angles, l and N , to the epoch "con-

stant" and "mean motion" in the expression of the Sun's longitude as a function of t ; and that the assigned values are not generally such that $A_0=l$; $N_0=N$. But I shall prove that not only is this the system of time-measurement adopted in stellar astronomy, but that it does not differ from the system which is adopted in gravitational astronomy unless the adopted value of the precession constant is in error, and that the two systems then only differ in the ratio

$$\frac{2\pi - P - \delta P}{2\pi - P} = 1 - \frac{\delta P}{2\pi - P},$$

where P is the adopted value, and $P + \delta P$ the exact value of the precessional constant referred to the mean tropical year as the unit of time.

The corrections for such differences, if sensible, can be duly allowed for at any time, as more accurate values of $P + \delta P$ become available, but these differences are altogether unimportant as compared with the differences due to the adoption of such different angles, N and N' for N_0 , as those in use before and after 1864.

It may, perhaps, avoid misconception if I point out that I am perfectly aware of the fact that it is possible to adopt other systems of time-measurement than that which I have here considered. It is possible to base our time-measures fundamentally upon the motion of the observer's meridian in right ascension by assigning definite values to the constants A_0 and Ω in the equation

$$2\pi t + A = A_0 + \Omega t + \sigma t^2 + \text{nutatation}.$$

If we assign values

$$0, l, l' \text{ to } A_0$$

$$2\pi, 2\pi + N, 2\pi + N' \text{ to } \Omega$$

we shall have three systems of *convertible times*, t_1, t_2, t_3 , where the "times" which correspond to a given right ascension, A , of the observer's meridian will be determinable from the equations

$$2\pi t + A = 2\pi + \sigma_1 t_1^2 + \text{nutatation}$$

$$2\pi t + A = l + (2\pi + N)t_2 + \sigma_2 t_2^2 + \text{nutatation}$$

$$2\pi t + A = l' + (2\pi + N')t_3 + \sigma_3 t_3^2 + \text{nutatation}.$$

If either of these systems was adopted and used to bring up the tabular right ascensions of the clock stars from epoch; and if the precessional motions and proper motions were expressed in terms of the same time-scale; there would be no difficulty in

determining the "local sidereal times" from observation by following a method similar in principle to that at present adopted. Taking the second system as an illustration, we should have if

$$A'_r = l + Nr + \sigma_2 r^2 + \text{nutation} - 2\pi(t - r)$$

$$t_2 = r + \delta r = r + \frac{A - A'_r}{2\pi + N},$$

and the equations for the determination of times with the observatory clock would be

$$\delta r_1 = \frac{\rho'(c_1 - c'_0)}{86400^s} \quad \delta r_2 = \frac{\rho'(c_2 - c'_0)}{86400^s},$$

where if the clock was the same as that before under consideration ρ' would not be the same constant, because the "unit day" of 86400^s is different, nor would c'_0 be the same as c_0 , because different instants of absolute time are denoted by the same number, r , on the two scales; but the equivalent of 86400 mean solar seconds in this case would be

$$86400 \left(1 + \frac{N}{2\pi} \right) \text{sidereal secs.};$$

and we should thus have from the equation

$$A'_r + (2\pi + N) \frac{\rho'(c_1 - c'_0)}{86400 \text{ mean solar secs.}} = \phi_1(r) + \phi', \quad \delta r_1 = \alpha_1 + \delta \alpha_1$$

$$A'_r + \frac{2\pi\rho'(c_1 - c'_0)}{86400 \text{ mean sidereal secs.}} = \alpha_1 + \delta \alpha_1$$

or

$$A'^s_r - \rho'c'_0 + \rho'c_1 = \alpha_1^s + \delta \alpha_1^s \\ \&c. = \&c.$$

The necessary adjustments between this system of time-measures and that which we have previously discussed would thus be thrown upon the clock-corrections

$$A'^s_r - \rho'c'_0 \text{ and } \rho',$$

and if this method was systematically carried out, the local sidereal times could be correctly found from observation. This method would possess the advantage that the equivalent in sidereal seconds of a "unit day" would be accurately known; and if the "unit days" of practical astronomy did contain

$$86400 \left(1 + \frac{N}{2\pi} \right) \text{sidereal seconds,}$$

the times found from the formula

$$\begin{aligned} t_2 &= r + \frac{\rho'(c_1 - e_0')}{86400 \text{ mean solar secs.}} \\ &= r + \frac{\rho'c_1 + \tau' + \delta\tau' - (\rho'e_0' + \tau' + \delta\tau')}{86400 \left(1 + \frac{N}{2\pi}\right)} \\ &= r + \frac{A^s - A_r'^s}{86400 \left(1 + \frac{N}{2\pi}\right)} \end{aligned}$$

would be thus correctly found.

But unless N happens to be identically equal to N_0 , and l equal to a definite but unknown angle l_0 , it will not be possible for $A_r'^s$ to be equal to A_r^s . It is easily seen that the intervals of time between instants marked by observational facts are correctly measured on all these systems of time-measures t, t_1, t_2, t_3 , when the proper allowances are made for the different intervals of time which are in these several cases called a "day," and put equal to 86400 seconds.

For if we wish to express the interval between the instants when

on the t_0 sidereal day from epoch, L.S.T. = A_0^s

and

on the t sidereal day from epoch, L.S.T. = A^s

we have

$$(t - t_0) 86400 \text{ mean secs.} = (t - t_0) 86400 \left(\frac{N}{N_0} + \frac{N}{2\pi}\right) \text{ sidereal secs.}$$

and this is identically equal to

$$86400 (t - t_0) + A^s - A_0^s.$$

And in the corresponding case of the system of time-measures, t_2 , we should have

$$\begin{aligned} (t_2 - t_0') 86400 \text{ mean secs.} &= (t_2 - t_0') 86400 \left(1 + \frac{N}{2\pi}\right) \text{ sidereal secs.} \\ &= 86400 (t_2 - t_0') + A^s - A_0^s. \end{aligned}$$

No errors, therefore, are made in measuring time by these different systems of time-measures, provided we do not forget that the "unit days" in the different systems denote different intervals of absolute times, and contain, therefore, different numbers of mean sidereal seconds.

It will, I hope, be thus clearly understood that I do not deny that the systems of time-measurement t_1, t_2, t_3 , and others might be adopted. I simply deny, as a matter of fact, that they are exclusively adopted in practical work.

If

$$\text{a "unit day"} = \frac{N}{2\pi} \text{ (of a mean tropical year)}$$

is used, we necessarily throw away for each count, one, of the "unit day," the true sidereal equivalent,

$$86400 \left(\frac{N}{N_0} + \frac{N}{2\pi} \right),$$

by the process employed for the determination of the errors of the sidereal clocks. If we used in practice

$$\text{a "unit day" } = 86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal secs.}$$

for the determination of the errors and rates of our clocks, we should necessarily throw away

$$86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal secs.}$$

as the equivalents for each count of a "unit day."

The local sidereal times of a meridian after r "unit days" on each scale are therefore necessarily different in the two cases by

$$86400 \cdot \frac{N - N_0}{N_0} \times r \text{ (sidereal secs.)}$$

and it is an allowance for the difference of this correction computed with the actual values of N and N' adopted for N_0 before and after 1864 which accounts for the *per saltum* change in the errors of Hansen's Lunar Tables, as compared with observation when the differences

$$86400 \frac{N - N_0}{N_0} \cdot r$$

and

$$86400 \frac{N_1 - N_0}{N_0} \cdot r$$

are neglected.

But even if it were true that the times as found from observation were on the scale of time-measurement, t_2 , it would not be generally true that these are the times required for the accurate determination of the constants contained in the differential equations and their integrals which represent the positions of the planets; and they would not, therefore, be the times required in practical astronomical work.

The general principle involved in these questions of time-determination is that definite values have to be assigned to some epoch correction, $n\tau$, and to some mean motion, n , in one of the theoretical expressions which represents a motion, which can be compared with the results of observations to render the time, t , observationally determinate.

The selection of a particular motion for this purpose is merely a question of practical convenience. The adoption of the mea-

tures t_1 , t_2 , or t_3 would simplify the direct determinations of the time "from observation, but would greatly complicate the mathematical investigations of gravitational astronomy; and this is the reason why they have not been exclusively adopted.

The differential equations of motion are formed on the assumed truth of Newton's law of universal gravitation, "that every particle of matter in the universe attracts every other particle with a force which is proportional to the mass of the attracting particle directly and to the square of the distance between the particles inversely."

The law thus generally stated is, however, of no practical value, as a basis of mathematical investigation, until we have defined how the masses are to be measured, and the relationship between the force of gravitation, the mass, and the distance practically established.

If we assume that the law of gravitation is true when the masses are directly measured by their accelerating effects, the law becomes available as a basis of calculation, and it is upon this assumption that the differential equations of motion used by astronomers are formed; but it will be found that when the masses are thus measured the measures themselves are not independent of the units of time and of space which are adopted, but change their numerical values with changes of these units; and if, to avoid this inconvenience, we agree not to measure "masses" by their accelerating effects, but to take the "mass" of the Sun as "the unit of mass," and denote the masses of the planets *Mercury*, *Venus*, *Earth*, &c., by the fractions

$$\nu_1, \nu_2, \nu_3, \&c.$$

which express the ratio of their "masses" to that of the Sun, so that the masses

$$I, \nu_1, \nu_2, \nu_3, \&c.$$

are independent of the adopted units of length and time, it will be found necessary, before we can form the differential equations of motion of the planets, to introduce a factor, f , such that

$$f \cdot I, f \cdot \nu_1, f \cdot \nu_2, f \cdot \nu_3, \&c.$$

will denote the accelerating effects of the respective masses. And, in this case, the numerical values which will have to be assigned to the factor f will depend upon the particular units of time and of length which we may adopt.

The factor f , when thus introduced, never appears in our equations unless multiplied by one of the masses

$$I, \nu_1, \nu_2, \nu_3, \&c.$$

and, conversely, a "mass"

$$I, \nu_1, \nu_2, \nu_3, \&c.$$

never appears in these equations unless multiplied by the factor f or as a ratio to other masses.

And it will be impossible, therefore, to correctly determine the numerical values of the fractions

$$\nu_1, \nu_2, \nu_3, \&c.$$

from the accelerating effects produced by the masses

$$f \cdot 1, f \cdot \nu_1, f \cdot \nu_2, f \cdot \nu_3, \&c.$$

unless the factor f has the same numerical value assigned to it in all our mathematical expressions, or unless any changes in the values adopted for f are accurately allowed for in evaluating the accelerating effects produced by the different masses.

The differential equations of motion for the different planets, the Moon, and for the rotation of the Earth about its axis are not isolated systems of three equations, but coexisting simultaneous equations.

And since the same symbols appear in many of these equations to represent the same physical quantities, it is absolutely necessary, in order that the results found from the integration of one set of equations may be transferable to the others, that the same units of length and of time should be adopted in the formation of all the equations of motion; or if different units are employed in special investigations, that all the physical quantities should be transferred to common units before they are used for the formation of any set of differential equations. And this will require that the ratios of the different units employed should be accurately known.

There is absolutely nothing to restrict our selection of units of time and of length in the mathematical investigations of astronomy, so long as no numerical value is assigned to f , and they are conducted in a purely symbolical form, and without the introduction of *any conditions which can only be satisfied by the adoption of particular units of time and length*. But to speak of the variable t in such cases as denoting "mean solar time" is nomenclature devoid of any physical significance. And if we adopt for the unit of time either the "mean sidereal day" or the "physical mean solar day," and for unit of length "a statute mile," "ninety-two millions of statute miles," or "the equatorial radius of the Earth"—units in terms of which the exact numerical value of the factor f is unknown—we shall have to overcome the mathematical difficulties which will arise from the appearance of an unknown factor, f , to all the measures of mass in our expressions.

But if $f_0 \times 1$ denotes the accelerating effects of the Sun's mass at the unit of distance in the unit of time, when such definite units as those described are adopted; and $f \times 1$, the value when units which bear to the former the ratio $1+p$, $1+q$ respectively

are adopted, it can be directly proved by a simple change of the variables in the differential equations of motion that

$$f = \frac{f_0(1+q)^2}{(1+p)^3} \text{ identically.}$$

If, therefore, we adopt for f_0 an approximate value, f' , it matters not in the slightest degree how f' may have been found, or why we have adopted it; we must, in order to correctly form the differential equations of motion in terms of definite units of length and of time, such as those above mentioned, substitute for f_0 its value

$$\frac{f' \cdot (1+p')^3}{(1+q')^2},$$

where $1+p'$ and $1+q'$ will denote the ratios of units of length and of time, for which the accelerating effects of the Sun's mass is equal to f' , to the definite units of length and of time in terms of which we propose to express all the linear and time-quantities found from observation, and to determine the numerical values of the constants which appear in our integrals.

If we put $p' = 0$ and $q' = 0$ and form the differential equations of motion with the definite value f' , then, if our mathematical work is accurate, and Newton's law of gravitation is true, the resulting integrals of the differential equations of motion will express the coordinates required in terms of such units of length and of time that for these units $f' \times 1$ does equal the Sun's accelerating effects at the unit of distance in the unit of time; but not in terms of the definite units of length and time in terms of which we proposed to express our linear and time quantities.

If we insist, therefore, upon the adoption of such definite units as those of the equatorial radius of the Earth and the "mean sidereal day" or the "physical mean solar day," we shall have, when using a definite value, f' for f_0 , to retain in all our equations of motion, and throughout all our mathematical work, the expression

$$\frac{f'(1+p')^3}{(1+q')^2}$$

for f_0 , and when we have succeeded in establishing exact relations between

$$\frac{f'(1+p')^3}{(1+q')^2}$$

and some linear quantity and some interval of time determinable from observation in terms of the selected units of length and of time, we shall be able to determine the numerical values of p' and q' , and thus deduce the required value f_0 .

But the numerical values of the numbers p' and q' found

in this way from a discussion of observations would be liable not only to fallible errors of observation but to serious systematic errors if any long inequalities

$$P \cdot \sin (\alpha t + \beta)$$

with sensible coefficients, P , were neglected in the theoretical expressions compared with the corresponding results from observation. For in such cases

$$P \cdot \sin (\alpha t + \beta) = P \cdot \sin \beta + \alpha t \cdot P \cos \beta - \frac{\alpha^2 t^2}{1.2} \cdot P \sin \beta \quad . \quad . \quad . \quad \&c.$$

and the effects of the terms

$$P \cdot \sin \beta; P \alpha \cdot \cos \beta, \&c.,$$

would be thrown upon the determinations of the epoch corrections and mean motions, and, therefore, upon the determinations of p' and q' , and of

$$f_0 = \frac{f'(1+p')^3}{(1+q')^2},$$

and the approximate values of f_0 thus found would be liable to systematic errors which would not even be constant but subject to determinate laws when found for different epochs, and from observations extending over different intervals of time. It is unnecessary to point out that the mathematical difficulties presented by the problems of physical astronomy would be enormously increased from the causes indicated if such definite units of time and of length were adopted. In such cases we should be practically replacing in our mathematical work the fundamental constant, f_0 , by a quantity which was a complicated function of the time.

It is possible, however, to avoid these mathematical difficulties with respect to variations in the adopted values of f by the selection of suitable units of length and of time; but if such units are selected to render f definite, then for accuracy all the linear quantities and time-quantities, which enter into our equations must be determined in terms of the same units as those for which f expresses the accelerating effects of the Sun's mass at the unit of distance in the unit of time; and if the unit of time is not equal

$$86400 \text{ sidereal seconds } \left(1 + \frac{N}{2\pi}\right),$$

then the use of the times t_2 , found from the equation

$$2\pi t + A = l + (2\pi + N) t_2 + \sigma t_2^2 + \text{nutation}$$

would certainly lead to error.

In fact, in these and other similar cases, the simplifications introduced in one stage of the work by the selection of particular

units are to some extent counterbalanced by additional complications introduced in the other parts of the work.

We shall next consider how the factor f , which is used in gravitational astronomy, is connected in practice with measures of time and of length, which fix the units in terms of which it will measure the accelerating effects of the Sun's mass at the unit of length in the unit of time, if Newton's law of gravitation is true. The conditions which are made use of in practice to fix the value of f are from the positions of the observing stations almost necessarily directly connected with the Sun's geocentric motion.

If the constants introduced in the integration of the differential equations which result from the neglect of the disturbing masses in the differential equations of the Sun's geocentric motion are denoted by $a, e, \omega, \tau, \Omega$, and γ ; and we denote by $a + \delta a, e + \delta e$, &c., the functions which when substituted for a, e , &c., in the expressions

$$R \cdot \cos L \cdot \cos \lambda = R [\cos v \cdot \cos \Omega - \sin v \cdot \sin \Omega \cdot \cos \gamma]$$

$$R \cdot \sin L \cdot \cos \lambda = R [\cos v \cdot \sin \Omega + \sin v \cdot \cos \Omega \cdot \cos \gamma]$$

$$R \cdot \sin \lambda = R \sin v \cdot \sin \gamma,$$

where

$$R = a (1 - e \cos \phi); \quad \tan \frac{v - \omega}{2} = \sqrt{\frac{1 + e}{1 - e}} \cdot \tan \phi$$

$$\phi - e \sin \phi = \sqrt{\frac{f' (1 + p')^3}{(1 + q')^2}} \cdot (t + \tau)$$

satisfy the differential equations of the Sun's motion, and the three subsidiary equations usually introduced to render the six functions

$$a + \delta a, e + \delta e, \text{ \&c., determinate,}$$

it is found that the geocentric longitude, L , and the distance, R , will be expressed under the forms

$$2\pi t' + L = \sqrt{\frac{f' (1 + p')^3}{a^3 (1 + q')^2}} (1 + \nu_3) \cdot \tau (1 + \delta \tau) + \sqrt{\frac{f' (1 + p')^3}{a^3 (1 + q')^2}} (1 + \nu_3) \cdot (1 + \delta P) \cdot t \\ + (2e_1 - \quad) \cdot \sin \zeta + \text{\&c.}$$

$$R = a (1 + \delta \theta) \left[1 + \frac{e_1^2}{2} - (e_1 - \quad) \cdot \cos \zeta + \text{\&c.}, \right]$$

where

$$e_1 = e (1 + \delta \phi)$$

is a constant, and t' is an integer.

The symbols $\delta \tau, \delta P, \delta \theta$, and $\delta \phi$ denote collections of small terms independent of the time which all contain, one or more, of the disturbing masses as a factor. The geocentric longitude, L , is here supposed to be measured from a fixed initial time.

If p' and q' have the numerical values assigned to them which renders the equation

$$f_0 = \frac{f' (1 + p')^3}{(1 + q')^2}$$

exact with the adopted value, f' , when the unit of length is the mean equatorial radius of the Earth, and the unit of time an interval which contains

$$86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal seconds,}$$

and if D denotes the mean distance of the Sun from the Earth, expressed in terms of the adopted unit of length, and n_s , l_s , the exact values, on the scale of time-measurement adopted, of the mean motion and epoch correction, we shall have

$$\sqrt{\frac{f' (1 + p')^3}{(1 + q')^2} \cdot \frac{1 + \nu_3}{a^3}} \cdot (1 + \delta P) = n_s \quad n_s \cdot \frac{\tau (1 + \delta \tau)}{1 + \delta P} = l_s$$

$$D = a (1 + \delta \theta) \left[1 + \frac{\theta_1^2}{2} \right].$$

But if n is the Sun's mean motion in the interval of time which contains

$$(1 + q') 86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal seconds,}$$

or in the interval for which, when adopted as the unit of time, f' is the exact value of the Sun's accelerating effects at the distance of the equatorial radius of the Earth, we shall have

$$n = n_s (1 + q'),$$

and if we adopt the interval of time,

$$(1 + q') 86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal seconds}$$

as the unit of time, instead of

$$86400 \left(1 + \frac{N}{2\pi} \right) \text{ sidereal seconds,}$$

we directly eliminate the factor $(1 + q')$ from our equations. In this case we have

$$\sqrt{\frac{f' (1 + p')^3}{a^3}} \cdot (1 + \nu_3) \cdot (1 + \delta P) = n;$$

but when this method is adopted, directly a value is assigned to n in the expression for the Sun's longitude, the unit of time is absolutely fixed; it will in this case be the

$$\frac{n}{2\pi} \text{ part of an interval of time, } \sigma,$$

where σ is generally called by astronomers the mean sidereal year.

Again, since

$$\sqrt{\frac{f'(\mathbf{I}+p')^2}{a^2(\mathbf{I}+q')^2}} \cdot (\mathbf{I}+\nu_3) \cdot \tau(\mathbf{I}+\delta\tau) = l_s = \frac{n\tau(\mathbf{I}+\delta\tau)}{\mathbf{I}+q'} = n\tau',$$

where τ' is an interval of time measured on the new time-scale, we can change the "epoch" or instant from which t is measured by putting

$$\tau' = \frac{l}{n},$$

where l is some assigned angle.

The times t then become determinate from the equation

$$2\pi t' + \mathbf{L} = l + nt' + (2e' \quad) \cdot \sin \zeta + \&c.$$

But we can further simplify the expression for the factor f , which expresses the accelerating effects of the Sun's mass at the unit of distance in the unit of time by adopting a suitable unit of length.

If we denote by D' the number which expresses the mean distance of the Sun in terms of a unit which equals

$(\mathbf{I}+p')$ times the equatorial radius of the Earth,

we shall have

$$D'(\mathbf{I}+p') = D = a(\mathbf{I}+\delta\theta) \left[\mathbf{I} + \frac{e_1^2}{2} \right],$$

and

$$\sqrt{\frac{f'}{D'^2} \cdot (\mathbf{I}+\nu_3) (\mathbf{I}+\delta\theta)^3 \left(\mathbf{I} + \frac{e_1^2}{2} \right)^3} (\mathbf{I}+\delta P) = n.$$

If, therefore, we take the unit such that

$$D' = (\mathbf{I}+\nu_3)^{\frac{1}{3}} (\mathbf{I}+\delta\theta) \left(\mathbf{I} + \frac{e_1^2}{2} \right) (\mathbf{I}+\delta P)^{\frac{2}{3}},$$

we have

$$\sqrt{f'} = n,$$

and, conversely, if we adopt n , the coefficient of t , in the expression of the Sun's geocentric longitude to fix the value of $\sqrt{f'}$, and assign a definite value to

$$l = n\tau \frac{(\mathbf{I}+\delta\tau)}{\mathbf{I}+\delta P},$$

the mathematical results obtained from the integration of the differential equations of motion when complete, or sensibly complete, will give the true coordinates of the Sun, Moon, &c., when the linear and time-quantities which they contain are all expressed in terms of the units of time and of length thus fixed. But the times t must in such cases be necessarily found subject to the condition

$$2\pi t' + \mathbf{L} = l + nt' + \&c., \quad . \quad .$$

where n is the angle adopted to fix the value of $\sqrt{f^1}$; or if the longitude L' is measured from the equinox, we shall have

$$2\pi t' + L' = l + (n + p + \delta p)t + \&c.,$$

where p is the adopted value, and $p + \delta p$ the true value, of the precessional motion on the scale of time-measurement under consideration. Comparing this with the expression adopted in stellar astronomy,

$$2\pi t' + L' = l + (n + p)t',$$

it follows that the time-scale adopted in stellar astronomy is not identically the same as that required unless $\delta p = 0$; but the differences are insignificant as compared with the differences between the times $t, t_1, t_2, t_3, \&c.$, and the necessary corrections can be easily and directly applied at any time if the error δp is found to be sensible.

But instead of fixing the unit of length in the way described, the following method is in practice more convenient, as it is generally employed in the deduction of the geocentric coordinates from the heliocentric coordinates.

Let the number p' be so selected that with the unit of length

$$(1 + p') \times \text{equatorial radius of the Earth.}$$

$$R' = \frac{a}{1 + p'} (1 + \delta\theta) \cdot \left[1 + \frac{e_1^2}{2} - (e' - \quad) \cos \zeta + \&c. \right]$$

becomes

$$R' = 1 + \frac{e_1^2}{2} - (e' - \quad) \cos \zeta + \&c.$$

We then have

$$a = \frac{1 + p'}{1 + \delta\theta};$$

and the equation

$$\sqrt{\frac{f'(1 + p')^3}{a^3} (1 + \nu_3) (1 + \delta P)^2} = n$$

gives

$$\begin{aligned} f' &= \frac{n^2 \cdot a^3}{(1 + p')^3 (1 + \nu_3) (1 + \delta P)^2} = \frac{n^2}{(1 + \nu_3) (1 + \delta P)^2 (1 + \delta\theta)^3} \\ &= n^2 \frac{n^3}{1 + \nu_3} \quad \text{or} \quad n^2 a_1^3, \end{aligned}$$

where

$$a = a_1 (1 + \nu_3)^{\frac{1}{3}} = (1 + \delta P)^{-\frac{2}{3}} (1 + \delta\theta)^{-1},$$

where a and a_1 will be linear quantities whose exact ratios to the adopted unit can only be found from the mathematical inves-

tigations conducted in terms of the unit of length thus fixed and with times, t , subject to the condition

$$2\pi' + L = l + nt + \&c.,$$

or

$$2\pi' + L' = l + (n + p + \delta p)t + \&c.,$$

when n has assigned to it the same numerical value as that employed in fixing the value of $\sqrt{f'}$.

The equatorial radius of the Earth must in such cases be expressed in terms of the adopted unit of length; but this presents no difficulty, for its numerical expression is not required when the equatorial horizontal parallaxes are introduced.

Any errors made in fixing the positions of the meridians in terms of the variable, t , due to the neglect of sensible corrections

$$\delta l \text{ and } x = \frac{N - N_0}{N_0}$$

will appear, from reflex action, as apparent corrections (but they will be mixed up with the effects of any neglected long inequalities), to the adopted epoch constant, l , and mean motion, n , in the expression of the Sun's longitude. We shall thus be able to ultimately determine the corrections δl and x ; but the effects of these errors will be much larger in the case of the Moon than in the case of the Sun during the progress of the approximations; and the errors thus introduced are not due to errors of gravitational theory, but to errors in the determination of the time.

This completes the proofs which are required to establish the necessity of the corrections which I have indicated in fixing the relative positions of the centre of gravity of the Sun and of the positions of the meridians in terms of the measure of time, t , which is adopted in practical astronomical work.

It has been shown that the system of time-measures adopted in gravitational astronomy is based on the geocentric motion of the centre of gravity of the Sun; that, subject to the possible small correction for error δp , the same system is adopted in stellar astronomy; that the local sidereal times are correctly found with this system of time-measures; but that errors are made in fixing the position of the meridian in terms of t or in finding the time on the required scale from the local sidereal times; and that these errors enter directly in the work in referring the tabular positions to the meridian for a comparison with the local sidereal times; and the "Tables" which I have given (*Monthly Notices*, vol. liv. No. 1) show that the differences thus made, before and after 1864, account for the *per saltum* change of the errors of the Tables which undoubtedly took place about 1864.

On Errors that may arise in Estimating Star Magnitudes by Photography. By Captain W. de W. Abney, C.B., R.E., D.C.L., F.R.S.

In a recent paper read before the Photographic Congress held in October last I gave my results of experiments carried out with a view of investigating the interdependence of time of exposure and intensity of light. I found that when very feeble intensities of light were allowed to act for times proportionately longer than when a greater intensity was employed, so that in all cases the product of intensity and time were the same, the chemical action, or in other words the density, produced was by no means the same under certain conditions of sensitive surface. Now, I have often expressed the opinion that it should be the same, but have guarded myself by adding within limits. For instance, five years ago, when President of Section A of the British Association, I stated this more explicitly.

I refer to this to justify myself against the charge of being inconsistent. I may also say that at the Paris conference on the stellar photographic map it was my intention to indicate the same, but I honestly say I was not prepared for such a break-down of the usually accepted law under some of the conditions under which the experiments were undertaken. The condition for a break-down is this, that if a slow plate be used—more particularly, I believe, if it be one which is a pure bromide—the products of time and intensity being a constant do not always give the same amount of chemical action. Now, this is a serious matter for the star-charts which are being prepared in accordance with the plans laid down at astro-photographic congresses, for it by no means follows that star magnitudes will always be the same when derived from different plates. I will show you a few examples on the screen of the results of the experiments made. In the first we have an example of a fairly rapid plate, which was exposed at varying distances from an amyl acetate lamp, with increased exposures for increased distances, so that the aforesaid product in every case was the same. It will be seen that the feebler the intensity of the light, the less the chemical action. The next slide shows this graphically, the transparency being measured from the negatives. It will be noticed that, in the case of the plate shown, when the intensity of the light is reduced to $\frac{1}{256}$, the apparent chemical action is only about $\frac{1}{4}$ what it should be, according to the ordinary assumption. The next slide gives the results in the shape of a table. Now let us apply this to the star magnitudes. Suppose such a plate be exposed so as to impress, say, a star of the 7th magnitude in a certain number of seconds; we should expect that to get one of the 11th

magnitude we should have to expose for an equivalent number of seconds. Yet if we gave that exposure we should find that we had impressed the image of a star no brighter than one between the 9th and 10th, since what I call the equivalent exposure would not be more than $\frac{1}{4}$ what it would be expected to be. If this be the case, it is evident that any star magnitudes which were obtained would be untrustworthy, unless special precautions were taken, which I can indicate briefly. The scale of gradation of a plate when exposed for the same time to varying intensities of light at the same time is altered, as I can show you by plates exposed in a sensitometer. For instance, we have two plates—one of the same make already shown, and the other a very rapid plate—the number of gradations as shown by the squares which have been acted upon by properly graduated intensities of light were fewer in the first case than in the second. Now, the last visible square in each may be supposed to represent the faintest light that would make an impression in the given time, so that if exposures with both of them were given for times which would impress them with the star-disc of, say, a 1st-magnitude star of a certain diameter, the limit of higher magnitude stars which would be impressed would differ considerably. If we take it that in one case stars from the 1st to the 11th magnitude, and in the other stars from the 1st to the 7th magnitude, were shown (the diameters of the star discs of the 1st-magnitude star being the same in both cases), then the diameters of the star discs of the 6th magnitude on the one plate and of the 4th on the other would be very approximately the same, supposing they lie in the part of the curve which descends in the straight part of the curve. In other words, there would be a proportional diminution in the diameters of the star discs in the latter plate. If, then, the discs of two stars largely differing in magnitude but lying within the limits indicated be determined, those of the others may also be determined, supposing a formula for decrease in diameter with increase in magnitude has previously been arrived at. It must, however, be recollected that there are portions of the curve which are beyond the limits indicated, and here any comparison would fail. There is, however, one comfort, which is that a very rapid plate shows less deviation from the assumed law than a slower one; in fact, the more rapid the plate, the less the deviation that will be shown. Further, I have come to the conclusion that the composition of the sensitive surface has much to say to the results. I wish to guard myself against any rigid expression of opinion on this subject, as my experiments are as yet incomplete, but I fancy that a homoioidide plate will be found to have less deviation from the assumed law than a homobromide. Those who are interested in the subject know that there have been many curious differences in the star magnitudes as derived from a

plate, and some observers have found that the time of exposure necessary to impress a higher magnitude star is greater than that which theoretically should be the case, and it seems probable that this result is due to the difference in behaviour of the plates used. It will probably turn out that each particular brand of plate will require a different formula for arriving at star magnitudes photographically, and that the formula which is suitable for one will not be suitable for another. I should like, in conclusion to this note, to point out a method by which an idea as to the suitability of a plate for stellar work can be obtained. In the investigations to which I have referred I also included the effect of a number of small exposures succeeding each other with varying rapidity, and I found that *the sum of a large number of small exposures was not equal to the same exposure given at one time*, and whether a plate were slow or rapid, it showed a break-down of the assumption usually made that such should be the case. Further, it was found that the longer the interval of rest between successive exposures the less the chemical action. The more rapid the plate the less this loss of energy appeared; in fact, it followed the rule found when the intensity of light was diminished. The plate placed in the lantern shows this, and the next slide shows the curve derived from it. It will be noticed how the chemical action falls off, whatever be the exposure.

Now, this affords a ready means of testing a plate, for if it fails largely with this test, it will fail also, to a certain extent, with the other. If a lamp of 1-candle power be placed at such a distance from the plate that 20 seconds' exposure gives a medium density to a portion of the plate, the remainder being covered up, and a sector with a 20° aperture be rotated in front of the beam some ten to twenty times a second for 360 seconds, a different part of the plate being exposed, then, should the densities given by the two exposures be within .7 of one another, the plate may be considered practically to obey the usually assumed law, and star magnitudes can be obtained from all such plates, as the times of exposure for obtaining higher magnitudes will very closely coincide with those which are theoretically required.

The Secondary Colour Aberrations of the Refracting Telescope in Relation to Vision. By H. Dennis Taylor.

It is notoriously easy to grow so accustomed to a supposed necessary evil as to become practically oblivious of its presence. The astronomer who diligently uses his refracting telescope ceases to notice the ever-present halo of purplish light which surrounds the objects of his scrutiny, and is somewhat startled,

or even offended, when some brother astronomer, accustomed to use his reflecting telescope, takes a look through the refractor, perhaps for the first time, at a planet or a star, and recoils with an expression of surprise at the unsightly amount of coloured fringe which he sees. And, certainly, to one who sees it for the first time and has always heard the refractor spoken of as *achromatic*, the secondary spectrum seems far too aggressive a fault to be ignored.

Much has already been written about the secondary spectrum of the refracting telescope, but, I venture to think, to relatively little practical purpose.

The theory of vision through the refractor has, so far as I know, never yet received adequate attention. At the same time, the production of an objective practically as achromatic as the reflector has in many quarters been looked upon as a mere refinement, because the very serious nature of the secondary colour aberrations of ordinary objectives, especially when their apertures are measured by feet, has not been sufficiently realised. I will now proceed to give what seems to me sufficient evidence of the detriment and loss of utility suffered by the larger refractors owing to the imperfect nature of their achromatic corrections.

Plate 1 illustrates the secondary colour aberrations of the refracting telescope in the form of graphic curves.

First of all, in fig. 1 is shown the colour curve of the great Lick refractor as plotted out from the actual measurements of the foci for the various colours, executed with great care by means of a large spectroscope by Mr. James Keeler.*

The great objective is so corrected that the two rays, B and F, come to exactly the same focus, which is taken as the zero in the following table, where the colour aberrations are preceded by a + sign when the respective colours focus beyond the focus for B and F, and by a — sign when they focus short.

TABLE I.

Lick Refractor. Focal Length = 57 feet.

Wave-length.	Fraunhofer line.	Distance of foci from focus for B & F. Inches.
·6867	B	0·00
·6562	C	— 0·24
·5889	D	— 0·45
About ·5650	Minimum focus	— 0·47
·4861	F	0·00
·4341	G' or H γ	+ 1·45
·4101	<i>h</i>	+ 2·76
	Visual focus	— 0·20

* *Publications of the Astronomical Society of the Pacific*, Nov. 9, 1890.

In fig. 1, as in all the other colour curves which I give, the wave-lengths are set off in arithmetical order along the perpendicular on the left hand; that is, the wave-lengths (expressed in thousandths of a millimetre) are measured by the ordinates. A scale, divided to inches and tenths, is set off along the base-line; that is, the variations in focal length for the rays of various wave-lengths are measured by the abscissæ.

Now, it seems to be a fact that large refractors are rather over-corrected compared to smaller ones. Otherwise they would show an aggressive amount of secondary red. We want a colour curve more truly representative of medium-sized refractors of average colour correction. Smaller objectives are generally calculated to refract the rays C and F to the same focus, but in practice I have good reasons for believing that the average state of correction may be said to be one in which the ray half way between B and C is united with F. The best state of correction is largely a matter of taste or individual eyesight; nevertheless, I have assumed the latter state of correction as the best to form the basis of a series of calculations, whereby the colour curve shown in fig. 2 has been arrived at. The Lick measurements are too few and too far apart to enable one to draw in the colour curve with a sufficient degree of reliability from one end of the spectrum to the other. Therefore I calculated out the colour curve (fig. 2) for a 2-foot object-glass of 30 feet focal length. The data on which I have proceeded are as follows:—

1. The very careful series of measurements of Refractive Indices for Ordinary Crown Glass and Ordinary Dense Flint Glass, executed by Dr. Hopkinson, and published in the *Proceedings of the Royal Society*, 1877 ("Refractive Indices of Glass"). These are perhaps the most accurate and reliable measurements of this sort ever executed.

2. It is well known that the refractive index for any ray of known wave-length can be expressed in terms of that wave-length with reasonable accuracy by means of Cauchy's formula—

$$\mu = a + b \frac{1}{\lambda^2} + c \frac{1}{\lambda^4} + d \frac{1}{\lambda^6} + \&c.,$$

where μ = the refractive index, λ = the wave-length, and a , b , c , and d , &c., are constants depending upon the physical properties of the particular glass in question. As Dr. Hopkinson points out, if we wish to calculate μ from λ with as great a degree of accuracy as may be obtained by actual observation, then it is necessary to work by the formula extended to four terms, as shown above. This I have done—first working out the values of a , b , c , and d for crown glass and flint glass respectively from the observed refractive indices for the four spectral lines as follows:—

TABLE II.

Ray.	Wave-length λ .	Ray.	Wave-length λ .
B	·6867	F	·4861
D ₂	·5889	H ₁	·3968

Next, I calculated the necessary values of $\frac{1}{\rho_1}$ for the crown lens and $\frac{1}{\rho_2}$ for the flint lens, which are necessary for refracting the ray half way between B and C (wave-lengths = ·6715) to the same focus as the ray F ($\lambda = \cdot 4861$) at a focal distance of 30 feet. Here, as usual, $\frac{1}{\rho_1}$ stands for the sum of the reciprocals of the radii of the crown lens, and $\frac{1}{\rho_2}$ for the sum of the reciprocals of the radii of the flint lens. Then, if F represents the principal focal length of the combination, we have

$$\frac{1}{F} = \frac{\mu_1 - 1}{\rho_1} - \frac{\mu_2 - 1}{\rho_2}$$

$$= \frac{a_1 - 1 + b_1 \frac{1}{\lambda^2} + c_1 \frac{1}{\lambda^4} + d_1 \frac{1}{\lambda^6}}{\rho_1} - \frac{a_2 - 1 + b_2 \frac{1}{\lambda^2} + c_2 \frac{1}{\lambda^4} + d_2 \frac{1}{\lambda^6}}{\rho_2}.$$

Let

$$A = \frac{a_1 - 1}{\rho_1} - \frac{a_2 - 1}{\rho_2}; \quad B = \frac{b_1}{\rho_1} - \frac{b_2}{\rho_2}$$

$$C = \frac{c_1}{\rho_1} - \frac{c_2}{\rho_2}; \quad \text{and } D = \frac{d_1}{\rho_1} - \frac{d_2}{\rho_2}.$$

Then

$$\frac{1}{F} = A + B \frac{1}{\lambda^2} + C \frac{1}{\lambda^4} + D \frac{1}{\lambda^6} \quad \dots \dots \dots (2)$$

or

$$A + B\lambda^{-2} + C\lambda^{-4} + D\lambda^{-6},$$

which is the formula expressing the reciprocal of the focal length for any ray in terms of the wave-length of that ray.

Differentiating (2) with respect to λ , it will be found that

$$d\left(\frac{1}{F}\right) = -2\lambda^{-3}(B + 2C\lambda^{-2} + 3D\lambda^{-4})d\lambda \quad \dots \dots \dots (3)$$

On equating $(B + 2C\lambda^{-2} + 3D\lambda^{-4})$ to 0, it will be found that

$$\lambda^{-2} \text{ or } \frac{1}{\lambda^2} = \frac{-\sqrt{C^2 - 3BD} - C}{3D} \quad \dots \dots \dots (4)$$

That is, the ray whose wave-length will satisfy equation (4)

will be refracted to the minimum focal length. (The sign of the root in (4) has been assessed from actual trial.*)

I will here give Dr. Hopkinson's Refractive Indices for the Crown and Flint Glasses respectively for the four selected rays or Fraunhofer lines :—

TABLE III.

Ray.	Crown.	Flint.
B	1.513624	1.615704
D ₂	1.517116	1.622411
F	1.523145	1.634748
H ₁	1.532789	1.656229

From these the following constants were obtained :—

TABLE IV.

Constant.	Crown.	Constant.	Flint.
a_1	= + 1.5031566	a_2	= + 1.5973804
b_1	= + .0053061	b_2	+ .0086972
c_1	= - .00021147	c_2	- .000084923
d_1	= + .00001742	d_2	+ .000027465

For a focal length of 360 inches for the F ray, it will be found that

$$\frac{1}{\rho_1} = \frac{1}{74.4292} \text{ and } \frac{1}{\rho_2} = \frac{1}{149.318}$$

on the supposition that the ray half way between B and C ($\lambda = .6715$) is refracted to the same focus as F.

From these data the objective constants, A, B, C, and D—see equation (2)—are found to be as follows :—

TABLE V.

A	= + .00275948
B	= + .000013045
C	= - .0000022725
D	= + .00000050085

* It is interesting to note that the use of Cauchy's dispersion formula carried to four terms (as above) only gives rise to errors in the value of μ not exceeding and generally less than .000010; which error, in the case of the crown lens, would give rise to an error of only .018 inch in a focal length of 360 inches, and an error of .009 inch for the same error in μ in the case of the flint lens. An error of .000010 in *observing* μ from a crown prism of 60° angle would imply an error in reading with the spectrometer equal to about three seconds of arc only. Therefore the errors likely to arise from the above application of Cauchy's formula are no greater than the errors likely to arise in taking observations of μ with the greatest care by means of the best of spectrometers.

From the above data the colour curve shown in fig. 2 was derived. The focal lengths of the rays of the spectrum A, B, C, D₂, E, *b*, F, G, *h*, and H₁, as well as six other rays of intermediate wave-lengths (where spaces seemed to require it), were calculated and gave the abscissæ of the curve. It will be noticed that its character is much the same as the actually measured curve of the Lick refractor, if we allow for the great difference of focal length. Some years ago Professor Piazzi Smyth was good enough to send to Messrs. Cooke a series of measurements of the focal lengths of rays of various colours executed with his great spectroscope, which consisted of a large diffraction grating, a collimating telescope of 4 in. aperture and 36 in. focal length, and an observing telescope of 4 in. aperture and 68 in. focal length. Allowing for there being two objectives in use, one being nearly twice as powerful as the other, and the nature of the colour correction, which brought the D and the G rays to the same focus, the colour aberrations as measured by him (if we allow for the difficulty of attaining great exactness) were found to be on a scale proportionate to the focal powers concerned. The colour aberrations of the 27 in. Vienna telescope have also been measured by Professor Vogel and found to be substantially of the same character as those of the Lick refractor, only less in amount in proportion to the smaller focal length. So far, then, calculation and actual observation are substantially in agreement.

We have now to consider an aspect of the question which is of very great practical importance, and also extremely interesting. Large refractors are looked upon as valuable because (1) of their supposed great light-grasping or space-penetrating power, and (2) because of their greater separating or dividing power, which enables them to deal easily with very difficult double, binary, and multiple star systems which are totally inaccessible to smaller instruments. Their greater separating or dividing power upon close double stars, &c., is of course fully proved, and is well known to increase directly as the aperture; it is chiefly the light-grasping power of refractors with which I intend to deal.

First of all, I will point out what seems to me to be a most inexplicable fact (?). It has been asserted with regard to the Lick refractor (in Table I.) that the eye does *not* select a focus at that point of the optic axis where the greatest concentration of light occurs, viz. at the *minimum* focus. That the eye is able to select any distinct focus whatever out of all the innumerable various coloured foci which are continuously distributed over several inches of length in the case of very large refractors is in itself a surprising fact. But there is found to be no ambiguity in practice about the exact whereabouts of the *visual* focus of large refractors. It is asserted by Mr. Keeler that the eye places the visual focal plane at a distance of

·27 inch, or about a quarter of an inch *behind* the position of minimum focus.

In fig. 1, f_1--f_1 is the visual focal plane as placed by Mr. Keeler. It is seen to cut the colour curve in two places—in the red near C, and in the bluish green between E and F. These two colours, being about complementary, together would form an apparently *white* focus. In fact the quality of *whiteness* and *not* luminosity would seem to be the eye's main requirement, if this visual focal plane is rightly placed. Now, when an optician designs an object-glass for visual purposes, he reasons thus: This objective has to yield an image which shall appeal as much as possible to our sense of vision; therefore it is of the utmost importance that those rays of the spectrum which exert the greatest effect upon vision should be refracted to the most compact focus possible; which amounts to this, that the visually most active rays must be refracted to the *minimum* focal length, so that the brightest part of the diagrammatic colour curve may be embraced tangentially by the focal plane. He pursues this sound line of reasoning, embodies it in practice, and gets the brightest yellow-green rays condensed to the most compact focus. And then (if Mr. Keeler's estimate of the position of the visual focal plane is the true one), when he comes to actually use the telescope, his eye most persistently ignores the very focus which has been specially designed to suit it, and selects a focus of its own, *far inferior in brightness*, at quite a different position on the axis.

This seems the very height of illogical perversity. As an example of discordance between design and use it compares very well with the well-known case of the planter who supplied his black labourers with wheelbarrows to lighten their toil, and found them next day carrying them, fully loaded, *on their heads*!

Professor Harkness, in an article on the Secondary Spectrum in the "Amer. Journal of Science and Arts," 1879, September, puts forward the following ingenious method of finding the position of the visual focal plane when the amount and character of the secondary spectrum are known.

In fig. 9 is shown the same colour curve as that of fig. 2 (for a 30-foot refractor). Across this colour curve there are drawn horizontally, so as to be bisected by the colour curve, a series of short lines whose length is made proportional to the visual brightness of the light at each point of the colour scale. After the ends of these lines have been connected we have the boomerang-shaped figure shown in fig. 9. (The figure as drawn does not pretend to any accuracy.) Let this diagram be drawn on stiff paper and then cut out and balanced upon a knife-edge (which, of course, must correspond to an ordinate). Professor Harkness maintains that the position of the knife-edge necessary for a balance should indicate the position of the visual focal plane.

Now, however applicable this solution of the problem may be in the case of small objectives where the secondary colour aberrations are not too great to be embraced between the limits of the focal plane, yet it is wholly inapplicable to the case of large objectives. For the image of a star is a spurious disc, of a well-ascertained diameter, and what we are really concerned with is the quantity or luminous value of the light which actually goes to the formation of that star disc. For, as I shall presently show, the light of certain other vagabond colours focusses at points so far beyond (if not short of) the visual focal plane as to form penumbrae or circles of aberration on the latter, which are so very much larger than the star disc that the light forming these said penumbrae is diluted tens of times, hundreds of times, and even thousands of times, according to the colour. Therefore, such light certainly does *not* sensibly contribute to the brightness of the star disc, and Professor Harkness's method is no longer applicable, unless applied *only* to that brightest part of the curve which can be comprised between the limits of the focal layer; a term which I shall now proceed to explain.

It is of the very greatest importance in this inquiry to know what are the limits between which the image of a point of light or a star may be said to be approximately *in focus* in the case of well-figured objectives. Fig. 5 embodies the generally, and I believe universally, accepted idea of the longitudinal section of the cone of rays from an objective or mirror converging to form a spurious disc at D, and then diverging again in a similar manner on the other side. It is supposed as a deduction from geometrical optics that the figure is that of two similar cones on a common axis, with their points overlapping by a distance equal to $2 \frac{F}{A}$ times the diameter D of the spurious disc. Here F represents the focal length, and A the aperture of the O.G. In fig. 5 I have represented the average case of the focal length being fifteen times the aperture. Let it be supposed that the star disc is viewed through an eye-piece powerful enough to magnify the spurious disc to a very appreciable size, as in the case of ordinary double-star work. Then, if fig. 5 is true to fact, it is evident that if the eye-piece is pushed within or beyond focus by an amount equal to the diameter of the spurious disc multiplied by $\frac{F}{A}$, or shortly $D \frac{F}{A}$, then it will focus upon a section of the cone of rays whose diameter d is equal to $2D$, and if pushed in or out by the amount $2 D \frac{F}{A}$, then it will focus upon a section of the cone of rays where $d = 3D$, and so on.

Now in the case of any objective where $\frac{F}{A} = 15$, it is well

known that the spurious disc is about $\cdot0004$ inch in diameter (or rather less). Therefore, according to fig. 5, if the eye-piece were pushed in or out of focus by a distance equal to $\cdot0004 \times 15$, or $\cdot006$ in., then the appearance should be a penumbra of light equal in diameter to *twice* that of the spurious disc. Now it happened to occur to me that I would determine the limits of good focus not only theoretically, by application of the usually accepted rule, but I would also confirm it by experiment. What, then, was my surprise when I found that, so far from there being any agreement between the tacitly accepted theory and actual fact, I actually had to push in the eye-piece or draw it out in order to expand the spurious disc into a penumbra of twice its size (see fig. 5a and b), by an amount equal to $\cdot03$ inch on *each* side of focus, or *five* times as much as I had theoretically expected, while I could push the eye-piece in or out by $\cdot015$, or nearly $\cdot02$ inch, without sensibly increasing the size or destroying the character of the spurious disc. This astonished me, and led me to make a series of experiments with different eye-pieces, apertures, and focal lengths, and varying relations between apertures and focal lengths. The results turned out to be independent of the focal lengths of the objective and the power of the eye-piece, provided the magnifying power was sufficient to show the star disc. Focal lengths of 9 inches (in which case the secondary spectrum is practically nothing) up to 11 feet were tried, with the results shown in Table VI. The first column gives the relation $\frac{F}{A}$; the second gives the whole distance of travel of the eye-piece between the point where $d = 2D$ on one side of focus and the point where $d = 2D$ on the opposite side of focus. Of course D increases as $\frac{F}{A}$, and theoretically the taper or angle of the cone of rays also varies inversely as $\frac{F}{A}$, so the *theoretical* distances of travel, including that on both sides of focus (for producing the condition $d = 2D$) given in the third column, are calculated as varying as $\left(\frac{F}{A}\right)^2$. The fourth column gives the ratio of actual to theoretical distances.

TABLE VI.

$\frac{F}{A}$	Actual.	Theoretical.	Ratios.
8.4	$\cdot02$ less	$\cdot004$	5+
15	$\cdot06$	$\cdot012$	5
30	$\cdot18$	$\cdot048$	3.75
60	$\cdot54$ about	$\cdot192$	2.8

These measurements do not admit of great accuracy; but so far

as they go they clearly prove that the taper of the cone of rays becomes much more gradual as the focus is approached, or as the diameter of the cone grows small in comparison with the wave-length.

Any observer can verify these results for himself on his own telescope. The discrepancy between calculation and fact is far too great to be explained away, and the conclusion is that fig. 6 pretty accurately represents the longitudinal section of the cone of rays near the focus when $\frac{F}{A} = 15$. Figs. 7a and 7b show

the differences between the theoretical and actual cones for a *large* relative aperture. I can scarcely suppose that I am the only observer who has noted this curious law, but I am not aware that the modification of the taper of a cone of rays near its focal point has ever been studied as a deduction from the undulatory theory of light.* But here we have a fact which renders possible what before seemed impossible, viz., the production of a tolerably definite focus out of the confusion of many foci; for since the bundle of rays is nearly cylindrical for a distance of nearly .02 on each side of focus (when $\frac{F}{A} = 15$), evidently colour aberrations to that amount on each side of focus will not give rise to

loss of light owing to any part of it falling outside the limits of the star disc. In all the colour curves shown in figs. 1, 2, 3, and 4 I have drawn what I call the focal layer, $f - f$, of a width in accordance with the above experiments. The focal plane may be indicated by a line drawn down the centre of the focal layer, while the outer dotted lines mark the limits of the focal layer. Supposing an eye-piece is focussed upon the focal plane, then the two rays of other wave-length which focus respectively upon the inner limit and the outer limit of the focal layer will produce upon the focal plane two penumbrae of light whose diameters are equal to twice the diameter of the focussed disc (see figs. 5a and 5b); that is, about *one-quarter* of the light which focusses upon the two limits of the focal layer will fall within, and contribute to, the brightness of the spurious disc. Any light focussing sensibly beyond the outer limit or sensibly within the inner limit of the focal layer will contribute a still less fraction of its illuminating power to the spurious disc or star image.

* It can be shown that no amount of residual spherical aberration can possibly account for this apparent drawing out of the cone of rays. In order to guard myself from being misled by the well-known variability of the focus of the eye of an observer, I made use of very high power eye-pieces for the greater number of the above experiments. The most powerful eye-piece used had an equivalent focal length of .15 inch, and in this case the utmost error in reading that could arise from the extremes of focal adjustment likely to take place involuntarily in my eye would not amount to more than $\frac{1}{300}$ th of an inch. For the reasons indicated above, the use of eye-pieces of equivalent focal lengths greater than about $\frac{1}{2}$ inch would be likely to give rise to very large absolute errors in the readings.

On careful consideration it will be conceded, I think, that the thickness of the focal layer, when defined as that thickness necessary for the condition that a ray focussing upon one of the limiting planes shall produce a penumbra upon the *focal plane* of a diameter equal to twice that of the star disc, errs if anything on the side of liberality; for the integrated values of the very small fractions of the light contributed to the star disc from the colours (if any) focussing at various distances outside limits of the focal layer can scarcely be supposed to balance the loss (rising upwards to 75 per cent.) of light incurred by the star disc as regards those colours which focus at points just *inside* the limits of the focal layer, and yet just outside the limits of sharp focus, or between the outer dotted and the inner solid lines in the diagrams. Hence the luminous value of the light of that range of wave-length $\Delta\lambda$ which is embraced between the limits of the focal layer may be taken as a fair and very liberal measure of the light-grasping power of the objective when used for star work.

Although the observations recorded in Table VI. do not admit of great exactness, especially when $\frac{F}{A}$ becomes great, yet they seemed to me reliable enough to prove that the thickness of the visual focal layer does not vary as $\left(\frac{F}{A}\right)^2$, but varies roughly as $\left(\frac{F}{A}\right)^{1.6}$, since doubling the ratio $\frac{F}{A}$ was found to be about *treble* the thickness of the focal layer (or rather more than that). Turning again to fig. 1, it will be seen that I have indicated one focal layer $f_1 - f_1$ just where the published measurements of the Lick refractor state it to be situated, its centre or the focal plane being at 27 inch *behind* the minimum focus. Now the curve L - L - L in fig. 1 (as in figs. 2, 3, and 4) is the curve of luminous intensity for the normal solar spectrum, the wave-length scale being exactly the same as that at the left-hand end of each diagram. For this luminosity curve I am indebted to the very valuable researches and experiments of Captain Abney and Major-General Festing, as published in the Bakerian Lecture for 1886.*

Since Fraunhofer's first attempt to measure the relative luminous values of the various colours of the solar spectrum, several systematic photometric investigations with the same object have been entered upon, especially those of Vierordt in 1869; but I venture to think there are no photometric measurements of the relative luminous values of the coloured constituents of white light which can be more safely relied upon than those embodied in the luminosity curves L - L - L here

* "Colour Photometry," *Phil. Trans.* Part ii., 1886.

presented. Captain Abney and Major-General Festing found their own eyes to be very fairly typical of the average results obtained by them when testing the colour-sensitiveness of other persons experimented upon.

They also well established the law that the visual photometric value of any mixture of colours is always equal to the simple sum of the separate visual photometric values of the separate coloured constituents, *whether the constituents are complementary or not*, and this supplies additional reason for doubting the existence of the supposed visual focal layer $f_1 - f_1$ (fig. 1) in the case of the Lick refractor.

I should remark that Captain Abney's luminosity curve is the curve for the *prismatic* spectrum, and gives the maximum intensity about wave-length $\cdot 576$ (citron yellow). On converting Captain Abney's curve into the curve for the *normal* spectrum with great care, I obtained the curve $L - L - L$ with the maximum intensity transferred to wave-length $\cdot 565$, citron green, in colour. It will be noticed that the Lick refractor is very exactly corrected to refract this most luminous colour to minimum focus.

Now in fig. 1 the outer limits of the focal layer $f_1 - f_1$ (placed in accordance with Mr. James Keeler's estimate of the position of the visual focal plane) cut the colour curve at two points near C in the red, and at two points near F in the greenish blue. If four horizontal lines are drawn through these points right across the luminosity curve $L - L - L$, then we get the two approximately rectangular strips $l - l$ and $l_1 - l_1$, and obviously the amount of light embraced by the focal layer will bear the same ratio to the whole light passed by the objective as the ratio which the combined area of the two strips $l - l$ and $l_1 - l_1$ bears to the whole area of the luminosity curve $L - L - L$. Hence it can be seen at a glance that if the visual focal layer is really placed by the eye at $f_1 - f_1$, then the amount of light concentrated in the star image must be only about *one-tenth part*, at most, of the whole light transmitted by the objective! This is far too improbable to be true, and therefore, as well as for other reasons already partly stated, I feel very sceptical about the position of the visual focal plane of the Lick refractor being at $f_1 - f_1$, as it has been stated to be. Let it be supposed, on the other hand, that the focal layer is really situated at $f - f$, nearly tangential to the colour curve. Then, on drawing horizontal lines from the two points where the colour curve is intersected by the outer limit of the focal layer, across the luminosity curve, we get a strip which, besides being very much broader, also includes the very brightest part of the spectrum; and it can be seen from a glance at the diagram that the luminous value of the light concentrated into the star image will be at least one-half of the value of the whole light transmitted.

Thus theory most certainly demands that the visual focal plane shall be so placed as to be nearly tangential to the bend of the colour curve.

It so happens that ordinary bright green glass, if of sufficient thickness, only transmits green light, whose maximum intensity is about wave-length 5400, very close to the point of minimum focal length for ordinary objectives. Any observer possessing a large refractor of at least 8 inches' aperture will find, as I have done, that if the focus is most carefully adjusted with a high power on a white star, and then the image is viewed through the green glass and a careful readjustment of focus made, that the eye places the focal plane for the unimpeded white light at about $\cdot 02$ inch only behind the green or minimum focus. I have also tried this method in the following case.

A 12.5-inch objective of 15 feet 5 inches focal length was arranged to be its own collimator; that is, it was placed close to, and with its optic axis perpendicular to, a plane optically-worked mirror. A small speculum metal bulb was placed in the eye end, just a little to one side of the principal focal point, which bulb produced an artificial star by reflecting the light from a very small electric lamp placed at one side. The light from this star diverged to the objective, passed out parallel, was reflected back by the mirror, and again converged by the objective towards a focus situated in the eye end close to the original star. This image, formed after two passages through the objective, could then be examined by ordinary eye-pieces. Now it scarcely needs explaining that, provided the speculum bulb is kept at an *unvarying* distance from the objective, then the colour aberrations (as well as any other faults in the objective) will be just *double* the colour aberrations of the same objective used in the ordinary way. So that by this method I practically had to deal with the secondary spectrum of a 25-inch objective of 30 feet 10 inches focal length, and its colour curve would be the same practically as that of fig. 2.

A knife-edge placed parallel to the focal plane, but worked backwards and forwards towards or from the objective by a micrometer screw of 100 threads per inch, was used for finding the exact positions of foci, which could be done with no greater liability to error than about $\cdot 007$ in. if the mean of several trials was taken. By this Foucault method I found that the focal plane, when the green glass was used, was about $\cdot 02$ in. within the focal plane selected by the eye when the white image was viewed without any coloured glass. About the same result was obtained when using the ordinary eye-piece method with and without the green glass.

These results seem to agree very well with what theory would indicate, and therefore I feel justified in placing the focal planes where I have done in all the diagrams. (I have placed

them a trifle ($\cdot 01$) too much towards the right in figs. 2, 3, and 4, but I have compensated for this by drawing the dotted lines going across to the luminosity curves from the proper points on the colour curves.)

Sir George Stokes some years ago devised a very simple and yet efficacious method of showing up in rather a surprising way the presence of the secondary spectrum in any objective. Fig. 8 serves to explain the principle of this test. An objective, $a - a$, is focussed very carefully upon a vertical white line drawn on a black ground. At the focus a small circle is shown, which serves as a cross-section, as it were, of the focussed line of light, which is, of course, supposed to be perpendicular to the plane of the paper. An opaque screen with its edge straight (and perpendicular to the paper) is now placed in front of the objective at $s - s$ so as to practically reduce its aperture to a semicircle. A little consideration will now show that any edge-rays focussing short of the previously-found focal plane will now strike the focal plane on the right-hand side, r , of the image, and that any edge-rays focussing beyond the same plane will strike the latter at the left-hand side, l , of the image.

It is found, in fact, that the right side, r , is conspicuously margined with yellow or citron green, and the left side, l , with reddish purple shading off into blue. Doubtless, the reason why this stray light is not noticed when the full aperture is in use is because in that case we get a combination of yellow-green and purple on each side of the image, and these colours, being roughly complementary (excepting perhaps in the case of very large objectives), form a greyish halo on each side, which naturally does not obtrude itself upon the attention so forcibly as the unsymmetrical combination of a citron-green halo on one side contrasted with a purplish and blue halo on the other.

Even if this test is applied to a 2-inch objective, these secondary colours may be easily detected. A black line seen against a white ground, such as the rod of a distant weather-cock, will answer equally well as a test object.

The above appearances, as exhibited by achromatics with the average degree of colour correction, clearly prove that the focal plane is not quite at the minimum focus, but are quite consistent with the position of the focal plane as I have shown it in the diagrams, where the inner *limit* of the focal layer is placed about tangential. If the visual focal plane were placed still further back, we should expect to see, under Sir George Stokes's test, a much greater amount of secondary green (as well as orange and bluish-green) in the case of large objectives than is actually seen. But the application of this test to an 8-inch objective did not seem to me to yield much more secondary green than the same test applied to a $3\frac{1}{2}$ -inch. Thus the visual focal layer seems to cling to the minimum focus, and if in the case of very

large telescopes it should really be proved to be placed further back by normal eyes, as at $f_1 - f_1$ in fig. 1, then our present theories of visual luminosities would have to be very radically overhauled in order to harmonise theory with fact, and to show *why* the eye should prefer in such an overwhelming degree the focus at $f_1 - f_1$ instead of the concentrated focus at $f - f$.

Table VII. gives roughly the estimated loss of light, represented by the *shaded* portion of the luminosity curves, incurred by objectives of the sizes named through the presence of the secondary spectrum.

TABLE VII.

Luminous value of whole light transmitted in each case	= 100
36 in. Lick refractor 57 feet, F. L., loss	= 27
24 in. refractor 30 feet, F. L., loss	= 42
12 in. " 15 " F. L., "	= 21
6 in. " 7.6 " F. L., "	= 9
28 in. Greenwich refractor 28 feet, F. L., loss = about 50.*	

Thus the Lick refractor has a great advantage owing to its relatively great focal length.

(1) If the aperture of a large objective of, say, 15 in. aperture is doubled while the focal length remains the same, it is evident that the colour curve remains the same while the light distributed along it is quadrupled in intensity at every point; but the focal layer is reduced to about $\frac{1}{3}$ rd, and, therefore, the range of wave-length $\Delta\lambda$ embraced by the smaller focal layer is reduced, but not reduced to quite so little as $\frac{1}{3}$ rd (to know exactly to what fraction would require a very precise knowledge of the colour curve near its minimum), but roughly to about one-half, and this half includes rather more than half the light, and the result comes out that doubling the aperture of large objectives while keeping the focal length constant does not quadruple the light-gathering power for star work, but only a little more than doubles it.

(2) If both aperture and focal length are simultaneously doubled, then it is evident that the abscissæ of the colour curve will be doubled, while the focal layer will remain of the same thickness; and it will now be found that the range of wave-length $\Delta\lambda$ embraced by the focal layer will be reduced to about $\frac{2}{3}$ rds; but the light in this portion of the curve is quadrupled in intensity, and the part actually embraced is of higher luminous value, and therefore the light-gathering power may be expressed as a little more than $\frac{2}{3}$ rds of 4, or, say, about 3. Thus doubling all

* For some kinds of astronomical work a relatively short focal length is highly desirable, but the above figures indicate that such a condition cannot be obtained without some sacrifice of that light-grasping power which should be appropriate to the aperture.

of the dimensions of the telescope will only increase the light-gathering power about three times instead of four times.

(3) If the aperture of a large objective is kept the same while the focal length is doubled, it is evident that the abscissæ of the colour curve are all doubled; but the thickness of the focal layer being about trebled, it will be found to embrace considerably more than the former range of wave-length $\Delta\lambda$, and the light-gathering power for star work will be accordingly increased (roughly, nearly $1\frac{1}{2}$ times)*

But it may be asked, Does not the light which *accurately* focusses in the star disc become quadrupled when the aperture is doubled as in case (1)? Certainly it does, but it should be borne in mind that, owing to the spurious disc shrinking to half its former size, light from the outer zones of the objective, which focusses a little within or beyond the disc, and which before contributed considerably to its brightness, now escapes it altogether, in that although the total light within the disc gains in one way yet it loses in another (see fig. 8).

When all the facts relating to this complicated question are taken into consideration, we can scarcely escape the conclusion that customary notions of the space-penetrating power of large refractors as compared with smaller ones need serious revision, and that all estimates of the relative magnitudes of stars based upon the customarily conceived relations between aperture and visibility are more or less misleading, especially if we take into consideration the large amount of variety in the colours of stars. For it is evident that the relative light-gathering power of the same telescope on a star in which yellow-green rays preponderate will be very much greater than its light-gathering power for a star in which blue rays or extreme red rays preponderate, since the blue-green and blue and violet rays are so hopelessly wasted, while the red rays about C and beyond are also much wasted.

Another important fact should not be overlooked. It is just in the case of small stars approaching the verge of visibility that any gain or loss of light is of the most vital importance. And there is a considerable weight of evidence to show that the relative luminous value of the blue and violet rays rises as the total illumination grows less† (Captain Abney's experiments do not, however, confirm this). Hence there is probably a greater loss of light in the case of small stars than in the case of bright ones, owing to the absence of the blue and violet, and therefore the reclaiming of the latter into the visual focus rises to greater importance in these cases.

* It would be unsafe to venture upon more accurate statements or formulæ relating to light-gathering power before this important subject has received yet further elucidation.

† *Modern Chromatics*, chap. xii., *Helmholtz's Popular Scientific Lectures*, p. 110.

Supposing it established that the visual focal plane is really situated close to the minimum focus in large telescopes, it might be urged that the images of stars should appear decidedly green or greenish yellow, and not white. This is partly the fact, at any rate in the case of small stars; but in the case of bright stars, the principle that the colours of the spectrum, and especially the brightest colours, tend to appear yellowish-white when raised to great intensity,* holds good.

The concentration of light into the star image is, of course, very great in the case of bright stars viewed through large instruments. And the fact that the halo of wasted light surrounding the star image is not more aggressive than it is is easily explained by the fact that such light is for the greater part so enormously diluted, and also by the fact that the light forming the star image has the advantage of comparatively great concentration, and stands out boldly by force of contrast. The purest white paper only reflects 50 times as much light as the deepest black paper under the same illumination; and as a matter of fact it can be shown that a photometric contrast of only twenty to one is to the eye as good as a black-and-white contrast.

It scarcely needs explaining that the effect of the secondary spectrum upon vision of objects having considerable extension, such as the planets or the Moon, is to throw a haze of light over the object; the straying colours corresponding to each point of the image are spread into a halo which overlies the images of surrounding points, and while no light can be said to be lost (in the case of the Moon), in a *luminous* sense, yet it is lost for *defining* purposes as certainly as in the case of stars. The image is a tolerably well-defined one, but having the appearance of being viewed through more or less mist. If the object shows various colours, as the belts of *Jupiter*, then the image may be likened to many coloured prints which one sees, in which the outline and details are rendered sharply in black, while the colours are sponged on anyhow; in the same way the image of *Jupiter* may be defined very well as regards outline and details by means of the most luminous green, yellow-green, and yellow rays; while the colours, such as the blue rays and the more extreme red, are spread on where required in a sort of diffused wholesale fashion. There is little to wonder at, then, in the fact that reflecting telescopes have got the reputation for defining coloured details more clearly than the larger refractors. It is the blue which is principally missing, and a number of experiments have been made which tend to show that the *colouring* power and the *defining* power for delicate details, of blue rays, is of a higher value than their small visual luminosity would lead us to expect.†

* *Modern Chromatics*, chap. xii.

† *Modern Chromatics*, p. 185; *Helmholtz's Popular Scientific Lectures*, p. 110.

I thought it would be of great interest to show the graphic colour curves (calculated in exactly the same way) for the 33-inch Lick Photographic Objective (about 47 feet F. L.), and also for the standard 13-inch Photographic Objective of 135 inches F. L. for charting the heavens. The focal layers are shown in both cases at the minimum bend. The large relative aperture of the 13-inch reduces the focal layer considerably. It will be seen at once that the focal layers embrace only a comparatively small portion of the range of wave-length to which the photographic plates are sensitive, and there is little doubt that the enlargement of the star images which takes place when either the stars are brighter or the exposures are increased may easily be explained by supposing that the aberrant rays begin necessarily to affect the plate at greater and greater distances from the true star disc. If this is the case, then, we should expect a marked difference between stellar photographs taken with reflectors and those taken taken with refractors. Of the former I have not sufficient information to enable me to confirm this point. If a curve of photographic sensitiveness were supplied in figs. 1 and 3 it would certainly be found that there is a considerable quantity of actinic light lost for defining purposes through the presence of the secondary spectrum, especially if we allow for the fact that the minimum part of the colour curve bends back and returns upon itself much more abruptly than in the case of the visual colour curve. This should be expected from the fact that the irrationality of dispersion between ordinary crown and dense flint glasses is greater as the blue end of the spectrum is approached.

It is abundantly evident that an objective which would refract all the colours as well as the blue actinic rays to exactly the same focus will possess more practical advantages than would appear at first sight. Hoping to introduce to this Society on another occasion an objective which will do this, and which is quite practicable up to reasonably large sizes at any rate, I must now bring to a conclusion a paper which has considerably surpassed the limits which I had at first intended for it.

Buckingham Works, York.

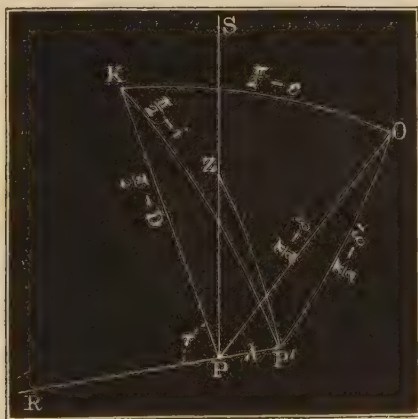
To adjust the Polar Axis of an Equatorial Telescope for Photographic Purposes. By Professor Arthur A. Rambaut, D.Sc.

An equatorial telescope intended for ordinary visual work may be considered to be adjusted with sufficient accuracy, as far as its polar axis is concerned, if the errors in altitude and azimuth do not exceed a single minute of arc. In most cases, too, the ordinary method of adjusting the axis by readings of the circles is not susceptible of higher accuracy unless elaborate precautions are taken to eliminate the effect of flexure.

When, however, a telescope is intended for photographic purposes it is advisable to orient the axis with a much higher degree of precision than this. In the *Bulletin du Comité International Permanent pour l'exécution photographique de la Carte du Ciel*, Tome 1^{er}, p. 385, Dr. Scheiner points out a method by which this may be accomplished. It has, however, the disadvantages that the corrections have to be made tentatively during the progress of the observations, and are affected by the variations in the refraction as the star changes its position.

In the method which I propose the errors in the position of the axis are determined by measures of a photograph on which two short exposures of a star have been taken at an interval of 15 or 30 minutes, the driving-clock of the instrument being allowed to run without hand correction in the meanwhile.

If the axis is not perfectly adjusted the two images will not fall on exactly the same point of the plate, and measures of the distance between the two images when corrected for the effect of refraction will afford the material for determining the position of the axis.



I assume, for the present, that the driving-clock is running accurately to sidereal time, and that the errors of the instrument have been reduced to within a few minutes of arc.

Let P be the pole of the heavens, and PS the meridian towards the south at S; Z the zenith; P' the pole of the instrument; K the point in which the extremity of the declination axis if produced would meet the sphere; and O the point towards which the telescope is directed.

Let the arc PP' be denoted by λ ,

"	"	KP'	"	$\frac{\pi}{2} - i$,	
"	"	KO	"	$\frac{\pi}{2} - c$,	
"	"	KP	"	$\frac{\pi}{2} - D$,	
"	"	ZP'	"	ζ ,	
"	"	angle SPP'	"	h ,	
"	"	SPO	"	θ ,	
"	"	KPR	"	τ .	

Then K describes a circle round P' and of course PP' is fixed. Let t_0 be the reading of the hour-circle when the point K is on the great circle PP', and t the reading when the telescope is directed to a point O west of the meridian. Then the angle KP'P is equal to $t - t_0$. We thus obtain from the triangle KPP'

$$\sin D = \sin i \cos \lambda + \cos i \sin \lambda \cos (t - t_0),$$

and $\sin \tau \cos D = \cos i \sin (t - t_0),$

or to the first order of small quantities,

and
$$\left. \begin{aligned} D &= i + \lambda \cos (t - t_0), \\ \tau &= t - t_0. \end{aligned} \right\} \dots \dots \dots (1)$$

We see also that θ is the hour-angle of the telescope. Let us put δ equal to the true, and δ' equal to the instrumental, declination of the point O; hence δ' is constant throughout the exposures.

If now the object-glass end of the telescope makes an angle $90^\circ - c$ with the declination axis, then $KO = 90^\circ - c$, and we have

$$\sin c = \sin D \sin \delta + \cos D \cos \delta \cos (\varpi + \theta - \tau - h),$$

or
$$c = D \sin \delta + \cos \delta \sin \left(\tau + h - \theta - \frac{\pi}{2} \right).$$

Hence unless the star under observation is within a few minutes of the pole

$$\sin \left(\tau + h - \theta - \frac{\pi}{2} \right)$$

is a small quantity, for which we may write simply

$$\tau + h - \theta - \frac{\pi}{2}.$$

Thus we obtain

$$\theta = \tau + h - \frac{\pi}{2} - c \sec \delta + D \tan \delta;$$

or, substituting from (1), we find

$$\theta - t = -t_0 + h - \frac{\pi}{2} - c \sec \delta + i \tan \delta + \lambda \tan \delta \cos (t - t_0) \quad . \quad . \quad (2)$$

whence

$$d(\theta - t) = -\lambda \tan \delta \sin (t - t_0) dt - [c \sec \delta \tan \delta + i \sec^2 \delta + \lambda \sec^2 \delta \cos (t - t_0)] d\delta.$$

Again, we have from the triangle PP'O

$$\begin{aligned} \sin \delta' &= \sin \delta \cos \lambda + \cos \delta \sin \lambda \cos (\theta - h) \\ &= \sin \delta + \lambda \cos \delta \cos (\theta - h); \end{aligned}$$

but from equation (2) we have, to a first approximation,

$$\frac{\pi}{2} + \theta - h = t - t_0,$$

and therefore

$$\sin \delta' - \sin \delta = \lambda \cos \delta \sin (t - t_0),$$

from which we find, to the same degree of approximation,

$$\delta = \delta' - \lambda \sin (t - t_0),$$

and consequently

$$d\delta = -\lambda \cos (t - t_0) dt.$$

We thus obtain

$$\left. \begin{aligned} d(\theta - t) &= -\lambda \tan \delta \sin (t - t_0) dt, \\ d\delta &= -\lambda \cos (t - t_0) dt. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (3)$$

and

Again, if ϕ denotes the latitude, we have

$$\cos \zeta = \sin \phi \cos \lambda + \cos \phi \sin \lambda \cos h = \sin \phi + \lambda \cos h \cdot \cos \phi.$$

Let $\zeta + k = 90^\circ - \phi$, *i.e.* let the pole of the instrument be too high by the angle k ; then

$$\cos \zeta = \sin (\phi + k) = \sin \phi + k \cos \phi,$$

and

$$\sin \zeta = \cos \phi - k \sin \phi.$$

Hence

$$k = \lambda \cos h.$$

If also we take A to be the azimuth of the point P' measured from the *north* towards *west* we have

$$\sin A = \frac{\sin \lambda \sin h}{\sin \zeta} \text{ or } A = \lambda \sin h \cdot \sec \phi.$$

Now, if the telescope is in the first instance pointed to a star, then $d(\theta - t)$ denotes the difference in hour-angle between the star and the telescope at the end of the interval dt , or $d(\theta - t) \cos \delta$ represents the displacement of the star's image on the plate in the direction of the parallel. Also $d\delta$ is the displacement at right angles to the parallel. Hence if we denote these displacements by X and Y respectively, we have the following equations:

$$\left. \begin{aligned} X &= d(\theta - t) \cos \delta = -\lambda \sin \delta \cos(\theta - h) \frac{15dt}{R}, \\ Y &= d\delta = \lambda \sin(\theta - h) \frac{15dt}{R} \end{aligned} \right\} \dots (4)$$

(where $R = 206265$),
 $k = \lambda \cos h$,
 and $A = \lambda \sin h \cdot \sec \phi$.

From these we obtain

$$\left. \begin{aligned} k &= - \left\{ \frac{X}{\sin \delta} \cos \theta - Y \sin \theta \right\} \frac{R}{15dt}, \\ \text{and } A &= - \left\{ \frac{X}{\sin \delta} \sin \theta + Y \cos \theta \right\} \frac{R}{15dt} \cdot \sec \phi, \end{aligned} \right\} \dots (5)$$

in which θ is the hour-angle of the star at the middle of the interval between the exposures.

With regard to the signs of the quantities k , A , X , and Y we see

- (α) That k is the angle through which the pole of the instrument must be *lowered*.
- (β) That A is the angle through which the *northern* end of the mounting must be turned towards the *east*.
- (γ) From the first of equations (4) we find that X is positive when $d\theta > dt$, i.e. when the instrument moves faster than the star. Hence the second image will fall to the *following* side of the first, or the *following* is the *positive* direction of X .
- (δ) From the second of equations (4) we see likewise that Y is positive when $d\delta$ is positive, or when the telescope at the end of the exposure is directed to a more northern point than at the beginning; consequently the second image will be *south* of the first.

Hence we see that taking as axes of X and Y , the projections of the parallel and meridian through the first image, and the plate being placed so that the stars on it appear as seen in an inverting telescope, the positive and negative directions of the

axes will be disposed in the usual manner as shown in the diagram.



The Refraction:—If the star whose image we are considering is not very close to the pole, the effect of the change in the refraction during the exposure must be taken into account when the highest accuracy is desired in the adjustment of the instrument.

This correction may be applied very conveniently by means of the formulæ which I published in the *Astronomische Nachrichten*, No. 3125. In the case before us we have $d\delta$ equal to zero, and da equal to dt ; and since we need only consider the terms of the first order the formulæ become

$$\Delta X' = \beta \cdot \frac{\cos \phi (\cos \delta \cos \phi + \sin \delta \sin \phi \cos \theta) 15 dt}{\{\sin \delta \sin \phi + \cos \delta \cos \phi \cos \theta\}^2 R}$$

and
$$\Delta Y' = \beta \cdot \frac{\cos \phi \sin \phi \sin \theta}{\{\sin \delta \sin \phi + \cos \delta \cos \phi \cos \theta\}^2 R} 15 dt$$

If, then, X' and Y' are the measured displacements along and at right angles to the parallel, we have

$$X = X' + \Delta X' \quad \text{and} \quad Y = Y' + \Delta Y'.$$

Having thus determined k and A by means of equations (5), the corrections may very easily be effected in the following manner:—

(1) *To adjust in altitude*, point the telescope to a star near the meridian and not very far from the pole. Set the star on the horizontal wire, and by means of the scale ordinarily found at the eye-end of the pilot or guiding telescope, displace the wire in declination through an angle equal to k . Using the adjusting screws at the base, bring the wire once more into coincidence with the star. The axis will then be correctly adjusted in altitude.

(2) *To adjust in azimuth*, point the telescope on a star near the meridian and not far from the equator. Having set the star on the vertical wire, displace the latter through an angle $D [= A \sin (\phi - \delta)]$. When the wire is brought once more into coincidence with the star by means of the adjusting screws at the

base of the instrument, the axis will be correctly adjusted in azimuth.

Irregularities in the Clock.—In what precedes I have supposed that the clock was running correctly to sidereal time.

In this connection there are two cases to be considered. If the clock is one of Sir Howard Grubb's latest pattern, in which the rate is electrically controlled by a sidereal pendulum clock, this condition will be fulfilled.

If, however, the clock is one whose rate may be depended upon to remain constant during the exposure, but without control from a pendulum clock, it may be rated to keep sidereal time by observation of a star at the equator. For the first of equations (3) shows that $d(\theta - t)$ is zero for such a star. Hence, in order to rate the clock correctly, it is only necessary to direct the telescope to a star on the equator and to adjust its rate until it keeps pace with the star.

But if the rate cannot be depended on to remain constant during the interval between the exposures, since the principle of the method will not allow of any hand correction being introduced, it will be necessary to reject the displacement in R.A. and to depend entirely on that in declination. We can, however, by properly selecting the stars, obtain both k and A from these displacements. For if in the first instance we take a star on the meridian, since θ is then equal to zero, we find from equation (5) that

$$A = -\frac{YR}{15 \frac{dt}{dt}} \sec \phi;$$

and if we next take a star at a distance of 6^h from the meridian, we have

$$k = \frac{YR}{15 \frac{dt}{dt}}.$$

The Observatory, Dunsink, Co. Dublin:
1893 December 1.

Note on a Star-correction Facilitator, or an instrument for readily obtaining the products of pairs of numbers, one number being taken from one set (e.g. day-constants) and the second from another set (e.g. star-constants). By T. C. Hudson, B.A. (Cantab.)

(Communicated by A. M. W. Downing.)

This instrument, which may for the present be briefly described as an antilog-roller with a set of four shiftable windowed sheaths, was primarily devised for the sake of minimising the labour involved in the calculation of star ephemerides. The principle may be explained as follows:—

I. If the first 100 natural numbers be written at regular intervals, thus,

0	10	.	.	.	.	90
1	11	.	.	.	.	91
2	12	.	.	.	.	92
.	.	.	.	.	.	.
.	.	.	.	.	.	.
.	.	.	.	.	.	.
.	.	.	.	.	.	.
9	19	.	.	.	.	99,

and if a pair of compasses be stretched from one number to another, and bodily moved about on the paper, up or down, right or left, but without altering the direction of the join of the points, then the *difference* of the numbers under the points will evidently remain the same whatever the numbers may be.

II. If these 100 natural numbers be replaced by the antilogarithms of '00, '01, '02, . . . '99, then the *ratio* of the quantities under the compasses will remain constant. This is evident from the fact that if the difference of two quantities be constant, so also is the ratio of their antilogarithms.

These considerations led to the construction of the instrument, which may be thus described.

On a cylinder are printed, or otherwise exhibited, at regular intervals, 100 columns of numbers, viz. the four-figure antilogs. of

'0000	'0100	&c.	'9900
'0001	'0101	&c.	'9901
'0002	'0102	&c.	'9902
.	.	.	.
.	.	.	.
.	.	.	.
'0099	'0199	&c.	'9999
'0100	'0200	&c.	'0000
.	.	.	.
.	.	.	.
.	.	.	.
.	.	.	.
'0199	'0299	&c.	'0099
'0200	'0300	&c.	'0100

the last column having, of course, the first for its right-hand neighbour. Thus the cylinder practically furnishes a complete antilog. table, rolled end to end, and combined with a similar

table placed beneath it and rotated one space to the left. The arguments '00, '01, . . . '99 are written above the top row, and '02, '03, . . . '99, '00, '01 below the bottom row. The vertical arguments may conveniently be written on a slip of card and applied to the cylinder when necessary.

On this cylinder is fitted a cylindrical sheath, capable of sliding up and down and of slipping round the cylinder. The length of the sheath is only 100 spaces, that is, half the length of the cylinder. This sheath is moved up to the top of the cylinder, so as to cover the upper half, and a zero mark is made over the antilog. of '0000. Then thirty-eight windows are cut in the sheath so as to disclose the thirty-eight day-numbers called *A*, or, more precisely, so as to disclose the thirty-eight antilogarithms of the logarithmic day-constants known as $\log A$. Finally, arrows are drawn from window to window showing the order of succession. Three other sheaths are made in like manner for the sets *B*, *C*, and *D*.

In order, then, to obtain the thirty-eight quantities called *cC* for a star whose $\log c$ is, say, 9.2764, we have merely to shift the "*C*" sheath till its zero mark is in column 27 and row 64, whereupon the thirty-eight windows immediately disclose the thirty-eight required quantities *cC* ready to be transcribed without either addition, interpolation, or the eye-wearying process of selecting the right number from the rows and columns of a large table. The number strikes the eye, without requiring to be selected from its neighbours.

The decimal significance of the antilogarithms is, of course, not given by the cylinder, just as an ordinary antilogarithm table does not furnish us with the position of the decimal point. But the value of the first quantity may be easily ascertained, and then the decimal significance of the rest of the numbers may be determined by keeping an eye on the differences, and by the consideration that the ratio of any two *cC*'s is the same for all stars.

Photograph of the Nebula H I 168 Ursæ Majoris.

By Isaac Roberts, D.Sc., F.R.S.

The nebula is in R.A. 10^h 12^m, Decl., 41° 57' north, and the photograph was taken on 1893 April 14, with an exposure of the plate during 4 hours. The scale of the enlarged photograph now presented is 1 millimetre to 24 seconds of arc.

The nebula is No. 3,184 in the *New General Catalogue*, and 2,052-2,053 in the *General Catalogue*.

Sir J. Herschel, in the *General Catalogue*, p. 83, describes 2,052 as pretty bright, very large, round, very gradually brighter

in the middle; and 2,053 as pretty faint, very large, round, very gradually brighter in the middle.

Lord Rosse (*Phil. Trans.* 1861, pl. 27, fig. 13, and *Observations of Nebulæ and Clusters of Stars*, p. 82) states that the preceding part is probably a portion of a ring. "The nebulosity connecting the three principal knots is very, very faint, but I have no doubt of its existence. The faint branch following the bright nebulosity is more doubtful."

The photograph shows this to be a very interesting spiral nebula, almost perfect in outline. In the centre of the spiral is a star of 14th to 15th magnitude, and around it are formed the convolutions, each of which is broken up into stars; four of them (omitting the bright star on the north side) are sharply defined, and the others, which are numerous, appear to be in all stages of development between very faint star-like patches and the defined ordinary star images.

There is still nebulosity between some of the spirals, as well as between the stars in the convolutions.

Several photographs of spiral nebulæ have from time to time been presented to the Society, and each one of them shows the spirals to be broken up into stars, or into star-like condensations, and I think the cumulative evidence thus brought before us amounts to a demonstration of the formation of stars by the condensation of nebulosity, or by the aggregation of meteoric or other cosmic matter.

Photograph of the Nebula H I 205 Ursæ Majoris.

By Isaac Roberts, D.Sc., F.R.S.

The nebula is in R.A. $9^h 14^m$, Decl. north $51^\circ 34'$, and the photograph was taken on 1893 April 12, with an exposure of the plate during 3 hours and 30 minutes. The scale of the enlarged photograph now presented is 24 seconds of arc to 1 millimetre.

The nebula is No. 2,841 in the *New General Catalogue*, and 1,823 in the *General Catalogue*.

Sir J. Herschel, in the *General Catalogue*, p. 79, describes the nebula as very bright, large, very much extended in the direction $150^\circ 8'$, very suddenly much brighter in the middle; equal to a star of 10th magnitude.

Lord Rosse, in his *Observations of Nebulæ and Clusters of Stars*, p. 75, states that it is like the nebula in *Andromeda*, with the nucleus a little nearer the following edge than the centre. I "think there is a small part of detached nebulosity which seems to have a connection with the other," and he gives a rough marginal sketch.

The enlarged photograph, and more clearly the negative,

show the nebula to be a symmetrical ellipse, with a distinctly stellar nucleus in the midst of dense nebulosity which surrounds it. Outside this is a well-defined zone of faint nebulosity, and then a broad ring, or zone, with little if any nebulosity in it. Outside this, again, is a very dense broad ring of nebulosity, and a patch of very faint nebulosity extends beyond the ring at the *s.f.* end.

The nebula is probably a circular or oval system seen in perspective elongated, and there are indications of condensations of matter of the rings.

The nebula resembles, on a very small scale, the great nebula in *Andromeda*, and the description given by Lord Rosse agrees well with the photograph, though he could not have seen the details which are there shown.

The Variable Spectrum of β Lyrae in the Region of F—h.

By the Rev. Walter Sidgreaves, S.J.

In the August number of *Astronomy and Astro-Physics* Mr. Keeler has given us an interesting account of his experiences of the variations of the spectrum of β Lyrae. All observations of progressive or recurring changes in the starry universe, made with the great refractor at Mount Hamilton, are necessarily of the highest value; and those changes which are indicated to us by the delicate linings of a spectrum band seem to have greater claims upon the light-capacity of the great glass.

Mr. Keeler has drawn attention to the main difficulty which besets all eye observations of delicate changes, in his remark that it is necessary to preserve a faithful memory of an appearance noted perhaps many days past. Photography, here, has the advantage of presenting to the eye simultaneously both the past and the present state of things. And, for this reason, Mr. Keeler does not claim for his notes a value at all equal to the photographic impressions of the spectrum of β Lyrae, obtained with much smaller optical power. We think that he under-rates the merit of his own work, and overrates the advantages of the photographic records. That these are considerable there can be no question. But it is not clear whether even greater allowance has to be made for the photographic plate than for the eye, under the diverse conditions of our treacherous atmosphere.

We cannot claim for the spectrographic plates of β Lyrae obtained at this observatory any such superiority over Mr. Keeler's notes upon his eye observations at Mount Hamilton. The star is only fairly within the working capacity of the 8-inch refractor on the clearer nights, and our recent examination of 45 plates shows that under these circumstances the

photographic plate may be more treacherous than the retina of the eye. The eye presents at least the same retina to the light rays, at all its observations; while the camera must change its plates. A little difference in the sensibility of the plate may greatly alter the value of the picture, when the light collector can barely provide sufficiency. An over-exposed plate can be dealt with fairly successfully in the process of developing; but an under-exposure can never be brought up to compare accurately with a plate that has had the advantage of the full action of light. Under-exposure has been the chief failing in the series of plates. It has not been possible to secure a full exposure on all occasions, owing to a defect in the clock movement as applied to the method of trailing. A small inequality in the thread of the driving-screw produces a periodic oscillation of a star about a mean position in the field when the clock is adjusted to true star time. And when the clock is slowed, the star travels across the field with a periodic variation of rate, and leaves on the photographic plate a spectrum band composed of a series of rulings. A slow trail, which corresponds to a long exposure, is shown on the plate by the closeness of the rulings; and the limit to the length of exposure is the rate of trail which gives an alternation between a complete halt and a march of the star's image; for if the image return upon its journey, the spectral lines must become blurred, unless the motion is more accurately parallel to the edge of the prism than can be secured without the use of a slit and constant attention to the instrument during the time of exposure. The difficulty of securing this adjustment for β Lyræ has been the cause of a number of failures; but the series of available plates numbers in all 45. Ten of these were taken in the spring and summer of 1892, on occasions of exceptionally clear nights, and the other 35 plates were the result of a set purpose to make as complete a collection as possible during the favourable position of the star in the last half of the present year. These were obtained between the third week of May and the middle of August, when the series came to an end with the last night of the 8-inch objective. The telescope was dismantled on August 16, in order to send its central box to Dublin to be fitted with the new tubes required for the Fr. Perry-Memorial objective. We may look for a more valuable record of the variations of the star's spectrum from the greater light power; but I have thought it well to offer the results of a very careful examination of the plates already obtained to the R.A.S. They may be confirmed or reversed by the more reliable plates of the future, but at least the comparison will show how far a weaker instrument can be trusted.

The spectrograph and its adjustments have been already described in the *Memoirs*, vol. li. p. 30. No alteration has been made in the instrument since its employment upon the

Nova of last year. The plates of β *Lyrae* have been arranged according to the day-age of its light variation, reckoned always from the preceding principal minimum.

In the accompanying map (Plate 2) the spectrum is given for each day-age, except day 10, for which there is no photograph, but only the more prominent lines have been marked. There are on some of the plates many more lines and bands which undoubtedly go through considerable changes. These have been omitted as not being sufficiently within the optical power of the instrument.

The wave-lengths are quoted from the curve constructed for the *Nova* of last year. They are referred to the position of G' at the marginal separation of its bright and dark representatives on the first days of the variation period, and the red side edge of the absorption line has been retained for the fiducial position for all plates not showing the red side radiation. The red side edge of F has also been employed as a second or corrective fiducial point on those plates which showed it with sufficient certainty. On some of the plates it was doubtful whether the line was F or a stranger, and on these plates it was not employed as a reference position.

It seems probable, from our examination of the plates, that the star's spectrum varies in general with its light period; for whenever there are more photographs than one for the same day-age, the same spectrum on the whole is shown on all the plates, making proper allowance for more and for less successful exposures. But there are exceptions which may eventually be brought into better harmony with some other period than that of the light curve. The most notable of these is shown in the map for the 11th day, which will be referred to again in our remarks upon the separate lines; and there may be others, if our estimation of the relative value of the photographs is much in error. But the terms maximum and minimum, as applied to prominence or strength of the radiation lines, do not appear to run with the corresponding epochs of the light curve. The maximum display of bright lines is between the 1st and 2nd day after the principal minimum, and the changes appear to be quite abrupt; for on the 11th and 12th days the bright lines are quite weak on good plates, and on the 13th day, a few hours before the principal minimum, the spectrum is still without bright lines.

Of the individual lines shown in the map, F is probably the one we should say least about, for the sensibility of the plates employed upon the star falls off rapidly at this region of the spectrum. But the contrast between the relations of F and G' in the spectra of β *Lyrae* and γ *Cassiopeiæ* is noteworthy; viz., that with the same instrument and the same plates, F is much stronger than G' in γ *Cassiopeiæ*, and generally much weaker in

β Lyrae. Its inferiority in β Lyrae may be a photographic deception; for it is possible that the actinic energy of the F rays may increase more rapidly with the increment of light than that of the rays of G'.

The companion lines at $\lambda 471$ seem to have a variation of their own, besides the general waning to extinction shown in the map. They are very strongly marked on the two best plates belonging to day 1; but they are wholly absent from the two very good plates of day 2, while a third plate of this day-age shows them quite clearly.

The group of lines at $\lambda 447-8$ is perhaps the most prominent feature in the spectrum. The stronger absorption line at the blue side fades away rapidly after the 3rd day, and the middle radiation line begins about the same time to divide, and gradually increases in the width and separation of the parts until the 11th day, when every part of the group is completely extinct on three plates belonging to this day-age, two of which are much too good to deceive. The reappearance of the absorption lines on the 12th day, without any bright companions, is also quite certain; two very good plates of this day-age bear witness to this, two ill-defined ones confirm them, and one inferior plate is doubtful.

$\lambda 438$ and G'. The radiation component of line 438 dies gradually from day 1 to day 4, when it is almost if not quite dead; while the absorption retains its full strength till the same 4th day, when it suddenly collapses with its companion. On the 7th day a new order has apparently just commenced; the absorption line has barely recovered, but the radiation has passed from its red to its blue side, and the same change has simultaneously commenced in the pair at G'. Both the components of G' have slowly lost their strength during the 4th, 5th, and 6th days; a more rapid decline has set in after day 6, and on day 7 the dark and bright companions appear faintly in the reversed order. The radiation is now on the blue side of the absorption. The absorption is narrow, and the radiation broad and undefined. Both are exceedingly weak, but are not doubted. On the 8th day the new order of the lines is quite manifest, and on the 9th they have attained apparently their maximum intensity, with the absorption quite equal to those of the first days, and the radiation about the same as on the 5th day. The same order is retained until the 12th day, the radiation fading slowly to nothing on this day, while the absorption remains unaltered till the 13th day, when it has suddenly vanished.

From this it appears that the reversal of the relative positions of the bright and dark components of G', and also probably of the weaker pair at 438, takes place at the secondary minimum, and again at the principal minimum, for the date of the 7th-day plate is ten hours after the secondary minimum, and this plate just

shows the change of position; and the date of the 13th-day plate is two hours before the principal minimum, and here both components of G are nearly completely obliterated. We have, however, the evidence of only one date for each of these days—the 7th and 13th; viz., two plates for the *same* 13th day, and a single plate for the 7th day. But their evidence has recently been confirmed by a fortunate trial plate upon β *Lyrae*, with the same spectroscope mounted upon the Fr. Perry-Memorial telescope. This plate is of the same light age as the 7th-day plate of the series, having been exposed at 9 hours after the secondary minimum, and it shows the same approximate obliteration of G' as is sketched on the 7th day of the map. Two other trial plates with the new objective confirm the succession of changes in G' as given by the series of exposures with the old 8-inch glass. One of these fills the gap of the 10th day, and shows the spectrum of the 9th day, with a small diminution of intensity in the absorption and radiation components of G' , so far as we can judge between the different conditions of the exposures; those of the 9th day being by slower trails with less light, and that of the 10th by a quicker trail and more light. The remaining trial plate was, as soon as developed, pronounced to be a 1st- or 2nd-day plate; and was found, on reference to the *Companion to the Observatory*, to belong to the 1st day, at 11 hours after the principal minimum.

The remaining line at $\lambda 410$ is at a weak part of the plate. The absorption is clearly seen on all the days except 7 and 8, and this change on the 7th day is confirmed by the stronger photograph already mentioned, which has it only as a mere ghost. No plate of the series shows the companion bright hydrogen in this region, but the stronger photograph of the 1st day by the new objective shows it clearly on the red side like its kin-fellow at G' .

In summarising our conclusions we give them as they come from our own photographs only, without reference to the conclusions drawn from observations with other and better instruments.

1°. It is quite certain that the hydrogen line at G' is at times double, one part being bright and the other dark.

2°. It is quite certain that the bright component is sometimes on the red side and sometimes on the blue side of the absorption line.

3°. It is highly probable that this change is periodical and coincident with the two minima of the light period.

4°. It is probable that the obliteration of both components at the times of the minima is due to neutralisation by their superposition.

5°. It is uncertain whether the bright component passes

across the dark one as a stationary line, or the two exchange positions.

6°. It is probable that the bright lines are strongest during the first three days of the light period.

The periodic change in the relative position of the two components of G' agrees very closely with the result of Professor Pickering's examination of the Harvard College photographs, as given in *A.N.* No. 3,051, and confirms his supposition of the binary nature of the star; but a rather elongated ellipse would suit the quickness of change at the minima of brightness, and the other requirements of our map, better than a circular orbit. The major axis being nearly in our line of sight, and the smaller and cooler star being between us and its primary at periastron, the continuous spectrum would be weakened by the partial eclipse; and the bright lines might be brought to their maximum intensity just after periastron by the heat developed in the tidal action upon the smaller star. They would at first be neutralised by the absorption lines while the star was crossing our line of sight, and would appear on the red side of the absorption, as soon as this rectangular inclination of the motion was sufficiently reduced. This would occur soon after the principal minimum, considering the high velocity at this part of the orbit and the short time-period of the revolution. The bright lines would retain their strength for some days, and then might cool down gradually to extinction before the next periastron. At the apastron there would be an eclipse of the smaller star, corresponding to the secondary minimum of brightness, and there would be a repetition of the superposition of the bright and dark lines, causing a temporary suppression of both, to be followed by the reversal of their relative position.

All these changes are faithfully followed by the hydrogen light at G', and the irregular appearances of other lines are to be expected from the nature of their origin in a tidal commotion. But at the precise epoch of a secondary minimum the dark hydrogen line should be visible without its radiant companion; and if this be discovered, it will greatly strengthen our confidence in this explanation of the variations of the spectrum of β Lyrae.

Stonyhurst College Observatory:
1893 December 1.

Two New Variable Stars. By the Rev. T. E. Espin, M.A.

On August 21, while sweeping for stars with remarkable spectra, I came across a fine red star which was found to be wanting in Argelander's charts. The place obtained for it was:—

R.A. $19^h 7^m 15^s.8$ Decl. $+25^\circ 46' 4''$ (1855).

The spectrum was banded, and believed to be III. type. As I suspected it might turn out to be variable, it was watched. On September 27 it was found to have diminished a magnitude. The following are my observations of it:—

1893.	Mag.		Mag.
Aug. 21	9.0	Oct. 11	11
Sept. 7	10.0	Nov. 13	11.5
Oct. 4	10.3	Dec. 1	12.0

On August 24, while sweeping in *Cygnus*, another fine red star was detected. It was near the place of a star of Argelander's, and was one of a wide pair. Careful differentiation on September 12 gave me for its place:—

R.A. $20^h 46^m 59^s.3$ Decl. $+46^\circ 47' 2''$.

It therefore is $18^h 7^m$ f. DM. $+46^\circ 30' 78''$, mag. 9.5. In the spectroscope the two star-spectra overlap, but it is evidently banded, and appeared more like type III. than IV. As this region had been swept over in previous years and no red star noted in this place, I concluded it was probably a new variable. It was not till October 4 that any change was noted, and even then the loss of light was so small that it would not have been seen except for the neighbouring star. The following are my observations of it:—

1893.	Mag.		Mag.
Aug. 24	9.1	Nov. 13	10.8
Oct. 6	9.4	Dec. 1	11.5
Oct. 11	9.6		

Note on the Spectra of Certain Red Stars.

By the Rev. T. E. Espin, M.A.

In the summer the mirrors of the $17\frac{1}{4}$ -inch were resilvered, and during the autumn the telescope has been used for re-examining the spectra of certain stars and for sweeping over the red region in *Cygnus*. I give herewith the results:—

(1) *Es. Birm* 57, R.A. $2^h 43^m.3$, Decl. $+ 56^\circ 34'$ (1900), Mag. 9.4. This is D.M. $+ 56^\circ 724$. This red star was found by Pickering, who suspected bands. Dunér at first believed its spectrum to be IV. type, but afterwards thought it III. My observations of it are 1891 December 22 in moonlight IV., mag. 9.2. (The neighbouring star D.M. $+ 56^\circ 725$, mag. 9.0.) December 29 III.!! Spectrum faint; mag. 9.0. It was re-observed 1893 December 1, when I was at once struck with its increased brilliancy. It was then quite equal to D.M. $+ 56^\circ 728$, mag. 8.5. The spectrum type III.!! The star is variable.

(2) *Anonymous*, R.A. $19^h 16^m.6$, Decl. $+ 37^\circ 41'$ (1900). A fine red star near G.C. 6791. It is 9.0 mag., and evidently banded, and, I believe, type IV. It was detected on 1893 September 16, and up to November 13 was found constant in light. Its absence from the D.M. is suspicious, and it may be variable.

(3) *Anonymous*, R.A. $20^h 5^m.0$, Decl. $+ 35^\circ 31'$ (1900). A red star, mag. 10, found 1893 August 14, and showing a banded spectrum, which is possibly type IV.

(4) *Es. Birm* 654, R.A. $20^h 7^m.3$, Decl. $+ 35^\circ 48'$ (1900). A very red star of type IV., found by Pickering, who gives mag. 12, and thought it possibly variable. It was observed 1893 September 16, mag. 11.0, type IV., and on several other nights, the last December 2, and no variation seen. It was found on two photographs taken with the 8-inch 1893 April 13 and October 16, and so far appears constant.

(5) *Anonymous*, R.A. $20^h 12^m.3$, Decl. $+ 37^\circ 34'$ (1900). A red star, mag. 10, found 1893 August 22. Banded spectrum, and possibly type IV.

(6) *Anonymous*, R.A. $20^h 13^m.3$, Decl. $+ 36^\circ 36'$ (1900). A red star, mag. 11.5, found 1893 September 16. Spectrum very faint, but pretty certainly banded, and believed to be type IV.

(7) *Es. Birm* 664, R.A. $20^h 17^m.6$, Decl. $+ 36^\circ 37'$ (1900). This is D.M. $+ 36^\circ 4025$, and is the p. star of two. This star was re-examined on August 14, and found not to be type IV. but type III.!! The magnitude of the star in the D.M. is 9.5, and my observations suggest variability in its light. Using D.M. $+ 36^\circ 4020$, mag. 8.4; D.M. $+ 36^\circ 4028$, mag. 9.5, as comparison stars, my observations are:—

1885.	Mag.	1890.	Mag.
July 2	9.7	Aug. 15	8.5
		1893.	
" 9	9.7	Aug. 14	8.4
1886.		Aug. 24	8.4
June 22	9.0	Sept. 16	8.6
Aug. 29	8.7		
1887.		Dec. 1	8.2
April 14	9.0		

On a photograph taken by Max Wolf, 1891, the estimated magnitude is 11.3, and on two photographs taken with the 8-in., mentioned under No. 4, the magnitudes are 12.5 and 12.2.

(8) RR. *Cygni*, $20^h 42^m.6$, Decl. $+ 44^\circ 30'$ (1900). As the spectrum of this star remains in doubt, it was examined on the nights of August 8, September 9, and was found to be III.!! The bands are large and ill-defined, but the character of the spectrum is undoubted.

(9) *Es. Birm* 706, R.A. $21^h 23^m.3$, Decl. $62^\circ 8'.4$ (1900). This is D.M. $+ 61^\circ 2134$. In consequence of there being some uncertainty about the type, it was examined 1893 September 30, and found to be type III.!!

(10) D.M. $+ 60^\circ 2432$, $22^h 40^m.4$, Decl. $61^\circ 12'$ (1900), mag. 8.9. This star was encountered in sweeping 1893 October 8, and found to have a spectrum of type IV.

(11) *Es. Birm* 735, R.A. $22^h 43^m.7$, Decl. $+ 54^\circ 38'$ (1900), mag. 9.5, D.M. $+ 58^\circ 2863$. On 1893 October 8 I examined this star, and found it had hitherto been wrongly identified as D.M. $+ 58^\circ 2865$. The spectrum is not IV. but III.!! My observations of its magnitude are:—

1887.	Mag.	1893.	Mag.
Oct. 20	8.5	Oct. 8	8.5
Dec. 4	8.2	11	8.3
Dec. 7	8.5	16	8.3
		Dec. 1	8.3

My observations are therefore very accordant, but still, the great difference between them and the D.M. magnitude suggests variation.

(12) *Es.* 141, R.A. $23^h 22^m.2$, Decl. $+ 48^\circ 58'$ (1900), mag. 9.3. This is D.M. $+ 48^\circ 4051$. The spectrum was observed for the first time on 1893 December 1, and found to be type IV., the three normal zones being well seen.

On the Orbit of a Centauri. By T. J. J. See, A.M., Ph.D.
(Berlin).

(Communicated by S. W. Burnham.)

The orbit of a *Centauri* has been investigated so frequently in the last quarter of a century that the elements of this famous southern binary ought now to be well determined. But owing to the discordance of the older observations, even the recent orbits of Elkin* and Powell† give periodic times differing by

* *Inaug.-Dis., Ueber die Par. von a Cent.*, p. 8, † *M. N.* vol. xlv. p. 19.

more than ten years, while the other elements are of necessity correspondingly uncertain. Downing* considers the orbit of Elkin (with a periodic time of 77 years) essentially correct, and vigorously contests the claims of Powell, who insists upon a period of 88 years, which in turn is supported by the researches of Doberck.

The great interest attaching to a *Centauri* as the first revolving double star ever discovered, the one binary system where the components are of the first and second magnitudes, and at the same time the nearest of all the fixed stars, renders the investigation of its orbit of such profound interest, as to justify research that will materially advance our knowledge of this celebrated system. After all that has been done on the orbit of this star, there is evidently no hope of obtaining results of real value without making the investigation as thorough as the material at present available will permit.

In beginning this research it was therefore decided to collect all the measures from original sources, with the number of nights on which the measures were taken, and the number of observations entering into each mean result. In some cases, unfortunately, these data had not been recorded by the observers, and hence could not be obtained; while in a very few cases the observations had to be taken on the authority of other astronomers who have published lists of observations.

The material obtained, however, certainly includes everything of any value hitherto published, and is ample for the deduction of an orbit substantially correct.

The first observations of conspicuous worth are the micro-metrical measures of Sir John Herschel, made during his residence at the Cape of Good Hope. The measures of Herschel taken in conjunction with measures recently made expressly for the purpose, have enabled us to determine the orbit of a *Centauri* with a degree of precision which is remarkable, when we consider the character of the observations. It will be found on inspecting the list of measures, that many of them are materially vitiated by errors of observation, some systematic, some accidental. In judging of the value of results, however, we must remember that a *Centauri* is a very bright star, so that the images are unusually large, and hence, unless the telescope is practically perfect and the atmospheric conditions are entirely satisfactory, it is hardly to be expected that the measures will be very accordant. We must also remember that there are no specialists in double-star work in the southern hemisphere, and hence we must not expect results such as could be obtained by the skill of a Burnham or a Struve. Of all the orbits hitherto found for a *Centauri*, only a few have now anything more than historical interest; some of the more recent elements are given in the following table.

* *M. N.* vol. xlv. p. 151.

Table of Elements of α Centauri.

P.	T.	ϵ .	α .	Ω	λ .	i .	Authority.	Source.
79	1863.25	0.818	"	"	32.7	"	Jacob	<i>Ast. Nach.</i> Bd. xliv. p. 43.
82.59	1857.01	0.96	31.76	2.6	27.65	77.3	Powell	<i>Mem. R. A. S.</i> vol. xxiv. p. 93.
75.3	1858.01	0.966	30.0	177.83	27.4	77.83	Powell	" " "
85.042	1874.85	0.667	21.80	21.6	59.53	82.3	Hind	<i>M. N.</i> vol. xxxviii. p. 97.
77.42	1875.97	0.526	17.50	25.8	54.8	79.52	Elkin	Dissertation, p. 8.
88.536	1875.12	0.533	18.45	25.54	45.97	79.4	Dobereck	
76.222	1875.95	0.516	17.33	25.51	54.99	79.25	Downing	<i>M. N.</i> vol. xlv. p. 239.
87.438	1875.45	0.5443	18.89	25.83	48.98	79.78	Powell	<i>M. N.</i> vol. xlvi. p. 337.
80.34	1875.74	0.526	17.20	25.22	52.5	79.53	Gill	<i>Mem. R. A. S.</i> vol. xlviii. p. 15.

In the following list will be found all the measures of α Centauri which are of any value, with the number of nights (n) on which the measures were made. The observations of T. Maclear, G. Maclear, and W. Mann, which were made about the middle of this century, are here taken from Dr. Elkin's *Inaugural Dissertation*, in which they were first printed; the number of nights on which these measures were made were kindly supplied by Dr. Elkin in a private letter. Most of the other measures are taken from the *Memoirs* and *Monthly Notices* of the Royal Astronomical Society.*

The writer desires to acknowledge his special obligations to Messrs. Tebbutt, Pickering, Douglass, Russell, Sellors, Gill, and Finlay for securing sets of measures of α Centauri expressly for the present research; also to thank Herr Hans Ludendorff, of the Royal Observatory, Berlin, for carefully confirming from the original sources the measures of Lacaille, Brisbane, Dunlop, and Johnson.

Observations of α Centauri.

t	δ	ρ	n	Observer.
1690'0	...	...	1	Richaud
1709'5	...	...	1	Feuillée
1752'20	218'73	20'51	...	Lacaille
1761'5	...	15'6	1	Maskelyne
1822'00	209'6	28'75	...	Fallows †
1824'00	215'41	22'45	35 +	Brisbane
1826'01	213'18	22'45	...	Dunlop
1830'01	215'03	19'95	...	Johnson
1831'00	215'97	22'56	...	Taylor †
1832'16	216'35	19'85	...	Johnson & Taylor ‡
1833'0	217'45	18'67	7 ±	Henderson
1834'33	217'33	17'83	1	Herschel §
1834'45	218'78	17'50	2	Herschel
1835'08	218'80	17'33	1	Herschel
1835'89	219'59	17'02	11 - 1	Herschel
1836'61	220'26	16'76	1	Herschel
1837'22	220'65	16'39	4	Herschel

* The writer intended to have given in this paper the original source of each measure and the number of observations on which it is founded; but, as these data render the paper voluminous and are not essential to our present purpose, they are reserved for a future publication.

† Taken on the authority of Herschel.

‡ See Powell, *Monthly Notices*, 1855-56, p. 171.

§ Herschel's means have been formed anew.

<i>t</i>	δ °	ρ "	<i>n</i>	Observer.
1840'00	223'2	14'74	...	Maclear
1846'21	232'4	10'96	3	Jacob
1846'80	234'3	9'56	4	Jacob
1847'09	235'7	9'33	2-3	Jacob
1847'36	234'5	9'31	3	Jacob
1848'00	237'93	8'05	13-12	Jacob
1849'63	244'5	6'23	...	Jacob
1849'94	245'25	6'96	1	Maclear
1849'97	245'42	7'04	3-2	Maclear
1850'10	246'63	7'01	1	Maclear
1850'17	245'75	7'08	6	Maclear
1850'20	245'85	6'84	3	Maclear
1850'31	247'07	6'75	4	Maclear
1850'37	247'52	6'52	7	Jacob
1850'38	245'74	7'12	1	Maclear
1850'41	242'0	7'78	15	Gilliss
1850'61	248'84	6'58	3	Maclear
1850'64	249'1	6'20	7	Jacob
1850'92	250'27	5'88	6	Jacob
1850'94	251'84	6'02	3	Maclear
1851'02	251'05	5'88	8	Jacob
1851'08	252'50	6'12	3	Maclear
1851'20	252'13	5'94	10-8	Jacob
1851'33	253'92	6'02	5	Maclear
1851'56	254'42	5'88	3	Maclear
1851'70	256'38	5'27	8	Jacob
1851'94	256'58	5'80	3	Maclear
1851'94	258'2	5'11	9-8	Jacob
1851'99	258'85	5'08	8-7	Jacob
1852'25	259'02	5'72	3	Maclear
1852'27	261'07	5'03	7	Jacob
1852'38	261'88	4'94	6	Jacob
1852'43	261'67	5'27	5	Maclear
1852'53	264'16	5'00	4	Jacob
1852'56	262'8	5'03	...	Maclear
1852'58	262'89	5'18	7-9	Maclear
1852'73	262'45	4'95	5-2	Maclear
1852'79	263'31	...	4	Maclear

<i>t</i>	δ	ρ	"	Observer.
1853'05	267°67	4'55	...	Jacob
1853'13	266°54	4'84	4-6	Maclear
1853'15	268°33	4'59	...	Jacob
1853'34	268°72	4'87	5	Maclear
1853'50	271°03	4'68	6	Maclear
1853'58	272°17	4'57	2-1	Mann
1853'58	270°10	...	...	Powell
1853'92	275°19	4'44	4-3	Maclear
1854°00	276°63	4'21	...	Jacob
1854°03	276°85	...	7	Powell
1854°24	278°98	4'62	4	Maclear
1854°25	279°06	4'16	2	Jacob
1854°26	279°62	...	4	Powell
1854°63	283°44	...	3	Powell
1854°66	282°81	4'43	5	Maclear
1854°93	285°88	3'96	5-4	Maclear
1854°96	288°02	...	2	Powell
1855°06	289°32	...	10	Powell
1855°23	290°19	4'38	3	Maclear
1855°29	292°60	...	5	Powell
1855°33	293°8	4'11	10	Powell
1855°36	291°96	4'38	4	Maclear
1855°54	294°73	...	5	Powell
1856°02	301°02	3'99	11-6	Powell
1856°02	302°13	3'85	7-6	Maclear
1856°10	303°06	3'88	18	Jacob
1856°38	306°92	4'05	1	Maclear
1856°51	309°84	3'93	10-9	Jacob
1856°91	311°26	4'21	4	Mann
1856°94	311°88	...	11	G. Maclear
1856°95	310°78	4'05	6	Mann
1856°96	315°77	3'96	10-9	Jacob
1857°15	318°19	4'02	15	Jacob
1857°39	320°60	4'47	2-1	Maclear
1857°86	326°48	4'14	14	Jacob
1858°17	330°51	4'39	5	Jacob
1858°23	339°42	5°09	3	Maclear
1859°34	339°71	5°18	15-12	Powell

<i>t</i>	δ	ρ	<i>n</i>	Observer.
1859.43	343.44	5.10	5	Mann
1859.52	341.8	4.92	4	Powell
1859.97	346.08	5.00	3	Mann
1860.05	346.55	...	1	G. Maclear
1860.09	345.4	5.65	17-13	Powell
1860.18	349.34	5.52	4-1	Maclear
1860.35	348.87	...	3	Maclear
1860.48	348.7	5.68	1	Powell
1861.05	351.08	6.07	10-9	Powell
1861.09	353.65	6.09	3	Maclear
1861.31	353.03	6.21	7	Powell
1861.58	354.26	6.32	5-3	Powell
1862.0	0.0	10.0	...	Ellery*
1862.20	357.84	6.80	7	Powell
1862.47	0.0	...	...	Ellery†
1862.56	1.38	7.55	3	Maclear
1863.03	1.4	7.2	6-4	Powell
1863.75	5.2	8.5	...	Ellery
1864.11	5.7	7.85	7-5	Powell
1864.72	...	8.1	...	Ellery
1865.56	17.3	9.95	1	Ellery†
1866.06	11.1	9.3	3	Powell
1868.17	...	9.2	...	Ellery
1868.18	...	9.6	...	Ellery
1868.38	13.59	10.29	2	Mann
1868.51	21.8	11.02	5	Ellery†
1869.13	17.97	10.4	2	Powell
1870.1	20.45	10.24	13-12	Powell
1870.61	21.8	10.09	5-4	Powell
1870.65	...	10.2	...	Ellery
1870.65	24.7	10.45	3	Ellery†
1870.75	22.53	10.46	4	Russell
1871.05	23.01	9.89	11	Powell
1871.31	23.7	9.8	7	Powell
1871.48	22.91	10.22	2	Russell
1871.51	24.2	9.41	1	Ellery

* Apparently a rough "guess."

† From transit observations.

t	δ	ρ	n	Observer.
1872.47	25.31	9.73	2	Russell
1872.55	24.1	10.36	1	Ellery
1873.16	...	8.3	...	Ellery
1873.33	28.1	9.50	1	Russell
1874.15	30.5	8.0	...	Ellery
1874.47	30.0	7.97	2	Russell
1874.85	34.17	...	...	Lindsay
1875.02	34.21	6.82	...	Seeliger
1875.94	39.3	6.68	1	Ellery *
1876.41	46.97	4.35	2	Russell
1876.61	51.05	4.15	2	Ellery
1876.90	64.3	4.94	1	Ellery
1876.94	51.2	4.5	1	Ellery
1877.14	64.4	3.30	...	Maxwell Hall
1877.25	69.1	3.13	5	Ellery
1877.52	72.77	2.60	2-1	Russell
1877.56	77.25	2.11	3	Russell
1877.57	80.50	2.13	2-3	Gill
1877.59	81.74	1.90	3	Russell
1877.63	81.49	1.94	3-1	Gill
1877.82	97.12	1.85	2-3	Gill
1877.89	101.12	1.62	2	Gill
1878.16	116.98	1.77	1	Russell
1878.22	119.82	1.95	3	Russell
1878.28	127.37	1.77	1	Russell
1878.38	139.10	2.40	...	Maxwell Hall
1879.25	174.40	3.41	...	Ellery
1879.47	173.55	3.41	2	Hargrave
1880.18	183.9	5.22	4	Tebbutt
1880.39	185.2	5.56	3	Tebbutt
1880.45	184.98	5.52	1	Russell
1881.28	189.88	5.07	1	Hargrave
1881.54	190.13	7.52	1	Hargrave
1881.65	193.15	7.94	2	Tebbutt
1882.00	194.44	8.23	18	Gill
1882.22	194.6	8.70	1	Tebbutt
1882.50	195.82	9.12	52	Elkin
1884.19	199.0	11.96	...	Russell

* From transit observations.

<i>t</i>	δ	ρ	<i>n</i>	Observer.
1884.43	199.5	12.32	...	Russell*
1884.53	199.80	12.93	6	Tebbutt
1885.56	200.8	14.05	4-3	Tebbutt
1886.27	202.5	14.89	5	Pollock
1886.38	200.4	14.74	1	Russell
1886.52	201.2	15.19	1	Russell
1886.55	201.02	14.87	4	Pollock*
1886.56	202.42	15.13	10	Pollock
1886.58	201.7	15.18	3	Russell
1886.60	201.41	15.16	4	Tebbutt
1887.39	202.3	16.06	3-5	Tebbutt
1887.43	202.08	15.83	6-5	Pollock
1887.60	202.35	16.28	3-2	Tebbutt
1887.72	202.16	16.18	2	Tebbutt
1887.74	203.0	15.73	4	Pollock
1888.30	203.4	16.87	3	Tebbutt
1888.63	202.93	17.12	1	Tebbutt
1889.45	204.5	17.91	3	Pollock
1890.41	205.2	18.58	2	Tebbutt
1890.47	204.75	18.66	4-3	Sellers
1890.60	205.05	19.06	3-2	Sellers
1890.74	204.6	18.69	1-3	Tebbutt
1891.43	205.62	19.15	5-4	Sellers
1891.56	207.17	19.25	4-2	Tebbutt
1891.57	205.3	19.24	2	Sellers
1891.64	206.4	19.35	3-6	Tebbutt
1892.30	206.45	19.52	2	Gill & Finlay †
1892.40	205.46	19.73	5-4	Sellers
1892.45	205.53	19.75	7-4	Tebbutt
1892.58	205.83	19.73	8-5	Tebbutt
1892.76	206.9	19.96	1	W. H. Pickering †
1893.21	206.9	20.04	1	A. E. Douglass
1893.21	206.75	20.22	2-1	W. H. Pickering
1893.34	206.4	19.92	8	Sellers
1893.42	206.73	20.32	6-4	Tebbutt
1893.49	206.5	20.24	8	Sellers
1893.50	206.75	20.53	4-2	Tebbutt

* From $\Delta \alpha$ and $\Delta \delta$.

† By photography.

After carefully inspecting the foregoing list of measures, we have formed the following mean results, which are referred to the meridian of 1900°0.

Mean Measures of a Centauri, with corresponding Computed places.

Epoch. <i>t</i>	Angle.		Distance.		No. Nights <i>n</i>
	δ_0	δ_c	ρ_0	ρ_c	
1690°0	...	257°62	...	5'23	1
1709°5	...	24°30	...	10°07	1
1752°2	217°84	216°66	20°51	18°30	...
1822°0	209°05	211°00	28°75	21°86	...
1824°0	214°88	211°90	22°45	21°57	35+
1826°01	212°66	212°83	22°45	21°10	...
1830°01	214°54	214°87	19°95	19°72	...
1831°0	215°49	215°42	22°56	19°29	...
1832°16	215°87	215°73	19°85	19°04	...
1833°0	216°98	216°62	18°67	18°33	7±
1834°33	216°87	217°50	17°83	17°63	1
1834°45	218°32	217°57	17°50	17°56	2
1835°08	218°35	217°70	17°33	17°46	2-1
1835°89	219°14	218°62	17°02	16°74	11-1
1836°61	219°82	219°17	16°76	16°30	1
1837°22	220°21	219°67	16°39	15°93	4
1840°0	222°78	222°30	14°74	14°11	...
1846°21	232°02	232°02	10°96	9°54	3
1846°80	233°93	233°47	9°56	9°08	4
1847°09	235°33	234°22	9°33	8°87	2-3
1847°36	234°13	234°92	9°31	8°67	3
1848°00	237°57	236°90	8°05	8°16	13-12
1849°63	244°15	243°02	6°23	6°93	...
1849°95	244°98	244°47	7°00	6°70	4-3
1850°20	244°97	245°70	6°92	6°52	14
1850°38	246°28	246°63	6°82	6°38	8
1850°41	241°65	246°77	7°78	6°36	15
1850°62	248°62	247°90	6°39	6°22	10
1850°93	250°7	249°65	5°95	5°99	10-9
1851°10	251°55	250°68	5°98	5°88	21-19
1851°44	253°33	252°85	5°95	5°65	8
1851°87	256°14	255°87	5°53	5°37	11

Epoch. <i>t</i>	Angle.		Distance.		No. Nights. <i>n</i>
	δ_0	δ_c	ρ_0	ρ_c	
1851.95	258°18	256°47	5'09	5'32	17-15
1852.33	260°58	259°43	5'24	5'09	21
1852.64	262°78	262°10	5'03	4'90	20-15
1853.27	268°73	268°07	4'69	4'57	17-18
1853.75	272°32	273°20	4'44	4'35	4-3
1854.16	277°91	278°00	4'33	4'19	13-6
1854.79	284°72	286°02	4'19	4'00	15-9
1855.25	291°26	292°28	4'29	3'91	32-17
1856.13	302°97	304°70	3'94	3'87	42-31
1856.51	309°84	310°05	3'93	3'91	10-9
1856.94	312°11	315°93	4'07	3'98	41-19
1857.27	319°09	320°27	4'24	4'07	17-16
1857.86	326°18	327°50	4'14	4'27	14
1858.17	330°22	330°99	4'39	4'41	5
1859.33	340°6	342°10	5'12	4'99	23-20
1859.52	341°52	343°80	4'92	5'11	4
1859.97	345°8	347°27	5'00	5'39	3
1860.27	347°78	349°40	5'62	5'58	26-15
1861.07	352°09	354°38	6'08	6'10	13-12
1861.44	353°38	356°50	6'26	6'36	12-10
1862.41	359°47	0°05	7'17	7'02	10
1863.03	1°14	3°57	7'2	7'44	6-4
1863.75	4°95	5°15	8°5	7°92	...
1864.11	5°55	6°33	7°85	8°16	7-5
1864.72	...	8°20	8°1	8°54	3±
1865.56	17°06	11°50	9°95	9°12	1
1866.06	10°86	12°75	9°3	9°32	3
1868.38	13°37	17°85	10°29	10°28	2
1868.51	21°58	18°10	11°02	10°31	5
1869.13	17°75	19°33	10°4	10°42	2
1870.1	20°24	21°20	10°24	10°47	13-12
1870.61	21°59	22°20	10°09	10°41	5-4
1870.65	24°5	22°28	10°32	10°40	3-5±
1870.75	22°33	22°66	10°46	10°35	4
1871.18	23°15	23°33	9°84	10°26	18
1871.49	23°35	24°15	9°82	10°10	3
1872.51	24°51	26°20	10°04	9°53	3

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of α Centauri.

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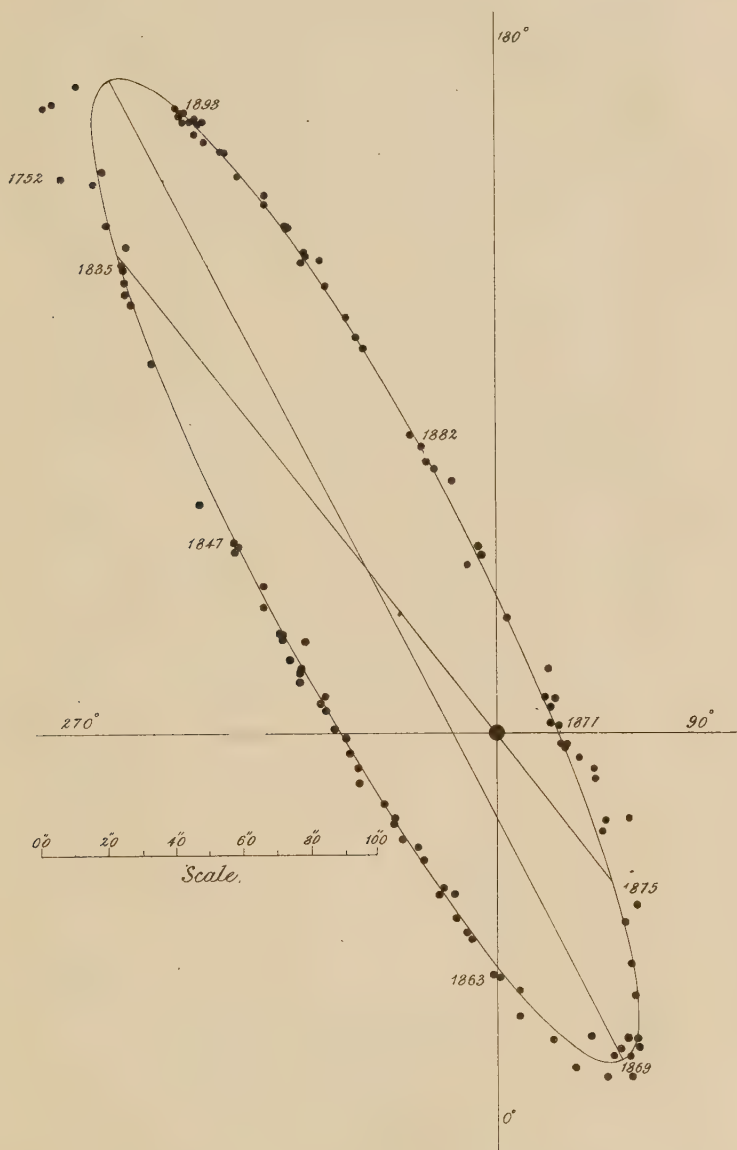
Epoch.	Angle.		Distance.		No. Nights.
<i>t</i>	δ_o	δ_c	ρ_o	ρ	<i>n</i>
1873'25	27°91	28°05	8'90	8'86	1-3 ±
1874'31	30°07	31°38	7'98	7'55	3-4 ±
1875'02	34°04	34°45	6'82	6'46	...
1875'94	39°13	40°68	6'68	4'83	1
1876'41	46°80	45°93	4'35	3'94	2
1876'61	50°89	48°97	4'15	3'56	2
1876'92	57°09	55°17	4'72	2'98	2
1877'14	64°4	61°23	3'30	2'59	...
1877'25	68°94	64°99	3'13	2'41	5
1877'52	72°61	76°85	2'60	2'01	2-1
1877'56	77°09	79°00	2'11	1'96	3
1877'57	80°34	79°58	2'13	1'95	2-3
1877'59	81°58	80°70	1'90	1'92	3
1877'63	81°33	83°06	1'94	1'88	3-1
1877'82	96°97	95°43	1'85	1'71	2-3
1877'89	100°97	100°93	1'62	1'67	2
1878'16	116°83	122°15	1'77	1'65	1
1878'22	119°67	126°78	1'95	1'68	3
1878'28	127°12	131°17	1'77	1'72	1
1878'38	138°95	138°05	2'40	1'80	...
1879'25	174°25	172°72	3'41	3'14	...
1879'47	173°41	176°90	3'41	3'56	2
1880'18	183°76	185°47	5'22	4'95	4
1880'39	185°06	187°15	5'56	5'36	3
1880'46	184°84	187°43	5'52	5'43	1
1881'28	189°75	192°13	5°07	7°06	1
1881'54	190°00	193°18	7'52	7'54	1
1881'65	193°02	193°58	7'94	7'75	2
1882°00	194°44	194°72	8'23	8'38	18
1882°22	194°48	195°35	8'70	8'77	1
1882°50	195°82	196°08	9'12	9'25	52
1884°19	198°89	199°32	11'96	11'93	...
1884°43	199°39	199°66	12'32	12'27	...
1884°53	199°69	199°75	12°93	12°36	6
1885°56	200°7	201°08	14°05	13°80	4-3
1886°27	202°4	201°83	14°89	14°67	5
1886°38	200°3	202°08	14°74	14°80	1

Epoch.	Angle.		Distance.		No. Nights.
t	δ_o	δ_c	ρ_o	ρ_c	n
1886.54	201°46	202°13	15''06	14''98	15
1886.59	201°46	202°87	15°17	15°04	7
1887.41	202°1	203°10	15°95	15°93	9-10
1887.69	202°31	203°57	16°07	16°22	9-8
1888.30	203°32	203°80	16°87	16°82	3
1888.63	202°85	204°38	17°12	17°12	1
1889.45	204°43	205°03	17°91	17°83	3
1890.49	204°93	205°20	18°77	18°63	9-7
1890.74	204°53	205°63	18°69	18°80	1-3
1891.50	206°01	206°03	19°25	19°31	14
1892.30	206°45	206°13	19°52	19°73	2
1892.40	205°40	206°15	19°73	19°79	5-4
1892.45	205°53	206°17	19°75	19°82	7-4
1892.58	205°83	206°23	19°73	19°86	8-5
1892.76	205°9	206°33	19°96	19°93	1
1893.21	206°9	206°53	20°04	20°03	1
1893.21	206°75	206°53	20°22	20°03	2-1
1893.34	206°4	206°56	19°92	20°18	8
1893.42	206°73	206°57	20°32	20°25	6-4
1893.49	206°5	206°65	20°24	20°34	8
1893.50	206°75	206°68	20°53	20°35	4-2

Ephemeris of α Centauri.

t	δ	ρ	t	δ	ρ
1894.50	207°22	20''81	1903.50	211°30	21''79
1895.50	207°70	21°16	1904.50	211°73	21°64
1896.50	208°17	21°44	1905.50	212°20	21°44
1897.50	208°62	21°66	1906.50	212°65	21°20
1898.50	209°07	21°82	1907.50	213°13	20°93
1899.50	209°52	21°92	1908.50	213°62	20°62
1900.50	209°98	21°96	1909.50	214°13	20°26
1901.50	210°40	21°95	1910.50	214°67	19°88
1902.50	210°83	21°89			

From these mean results the orbit will now be deduced. The observations are accurately platted on large drawing paper by means of a protractor, and we thus obtain a series of points through which the ellipse must pass as a sort of interpolating curve. Errors of observation of course prevent the points from



α Centauri

falling exactly on the arc of any true ellipse, and all that can be done is to pass the ellipse through the points so as to conform to the best observations. If the ellipse is judiciously drawn, we shall thereby eliminate not only accidental errors (as in the method of Least Squares), but also to a considerable extent free the measures from systematic errors, which are in general so troublesome in the adjustment of observations, and for the elimination of which the method of Least Squares is of no avail. By inspecting the apparent orbit, we see that the ellipse (which has been drawn with the greatest possible accuracy) satisfies the observations as a whole remarkably well; and in the deviations of the observations from the ellipse, we see the visible effects of systematic errors. The systematic errors of distance are conspicuous between 1822 and 1835; 1863 and 1870, where Ellery's distances are evidently too large; and 1875 and 1880, where the distances are generally over-estimated. There are analogous systematic errors in the position angles at different points of the orbit, but these can best be seen by inspecting the table of computed and observed places. The accompanying ellipse represents the observations as satisfactorily as could be desired, and therefore we do not hesitate to affirm that the elements deduced from this apparent orbit will be as good as can be obtained at the present time.* The elements here adopted have been determined directly from the apparent orbit by the method of Klinkerfues. In the present state of double-star astronomy, it is superfluous to attempt corrections of a very minute character, because the range of uncertainty is always considerable and there is no means of determining the true orbit with absolute rigour, since all the observations are necessarily more or less erroneous.

The following are the

Elements of α Centauri.

$P = 81.07$ years	$i = 79^{\circ}.74$
$T = 1875.62$	$\lambda = 51^{\circ}.56$
$e = 0.52$	$a = 17''.705$
$\Omega = 25^{\circ}.45$ (1900.0)	$n = +4^{\circ}.44184$

Apparent orbit:

Length of major axis = $32''.50$
Length of minor axis = $6''.16$
Angle of major axis = $27^{\circ}.2$
Angle of Periastron = $38^{\circ}.0$
Distance of star from centre = $5''.94$

* The reader is referred to a paper in *Astronomy and Astro-Physics* for 1893 December, in which the writer shows that in general a Least-Square

If we adopt the parallax of Gill and Elkin ($0''.75$), we find that the major axis of the orbit of *a Centauri* is 23.592 astronomical units; from which it follows that the combined mass of the components is 1.998 times the mass of the Sun and Earth, or sensibly twice the mass of the Sun. Thus we see that the companion of *a Centauri* moves in an orbit with a major axis which is about a mean between those of the planets *Uranus* and *Neptune*; but the orbit is so eccentric that in *Periastron* the two stars are only slightly remoter than the Sun and *Saturn* (11.3), while in *Apastron* the distance considerably surpasses that of *Neptune* from the Sun (36.0).

According to the preliminary reseaches* of Stone the masses of the two components of *a Centauri* are approximately equal. As it would be a matter of great scientific interest to determine this mass-ratio by means of more recent data, it had been the intention of the writer to embody the results of such a research in the present paper, but as new measures of the positions of the components are now being made at the Cape of Good Hope, it will be advisable to reserve the investigation of the centre of gravity for some future occasion. The great cosmogonic interest attaching to binary systems ought to induce astronomers to determine wherever possible the mass-ratio of the components, as the results will throw important light upon the problem of the formation and development of double stars. If the relative masses and the eccentricity can be determined with the desired degree of precision, the data thus obtained will not only be useful in fundamental astronomy, but will also furnish a firm foundation for future research in stellar cosmogony.

In conclusion, the writer desires to acknowledge his special obligations to Mr. J. W. Froyley, M.Sc., Fellow in Astronomy at the University of Chicago, for valuable assistance in the calculations embodied in this paper; also to thank Mr. S. W. Burnham for generous advice and assistance in collecting the long list of observations.

The University of Chicago :
1893 August 1.

Note.

Since the foregoing paper was finished, an important article on the orbit of *a Centauri*, by Mr. A. W. Roberts, has appeared in the *Astronomische Nachrichten*, No. 3,175. The results are almost identical with those obtained here, but as the two sets

adjustment is impracticable (owing to the absence of condition underlying the theory of Least Squares), and that it is also inadvisable to employ graphical interpolating curves to correct the angles or distances. The course here adopted is there fully discussed and justified.

* *Monthly Notices*. 1875-6, p. 259,

of elements are independent of each other and were obtained by very different methods, the coincidence can only indicate that the elements found are very near the truth. We may confidently believe that the orbit of *α Centauri* is now as well known as that of any binary star in the heavens. If in the course of time any corrections shall be required for the elements given above, it is certain that the changes will of necessity be very small.

The University of Chicago :
1893 August 30.

The Periodic Variation in the Motion of 61 Cygni.
By Harold Jacoby, B.A.

The important discovery by Dr. Wilsing* of a periodic variation in the distance of the components of 61 Cygni is of so great interest that it would seem desirable to re-examine the Oxford measures, to see whether additional evidence can be found. The late Professor Pritchard did not measure the distance between the two components directly, as was done at Potsdam. But in his parallax determination he employed two comparison stars (*a* and *b*), which lie very nearly on the great circle joining the two components of 61 Cygni. As the distance from each of these comparison stars to such component was always independently measured, we have but to perform a subtraction to get what is very nearly the distance between the two components. In fact, Professor Pritchard himself computed such "differences" for another purpose, and they are printed in his *fasciculus* iii., p. 24, for star *a*, and p. 37 for star *b*. They ought to exhibit a variation similar to that found by Dr. Wilsing from the Potsdam plates. In examining whether such is the case, I have thought it best to treat the results from stars *a* and *b* independently. Professor Pritchard gives also the excess of the mean of all the "differences" over each individual one. I have united these into general means; and to avoid any bias in selecting the several groups for this purpose, I have taken each calendar month by itself, except that the plates of 1886 May 26, 28, 30, and July 1, were included in the group for 1886 June. These were the only plates made in those two months, and their number was too small to use them separately. It is evident that the numbers so obtained will be positive when the distance of the components of 61 Cygni is small, and negative when it is large, as compared with its mean value. In this way I find the fol-

* *Kgl. Akad. d. Wissensch. zu Berlin, Sitzungsber.* xl., p. 879, 1893 Oct. 26.

lowing as the excess of the mean distance over that actually observed.

1886.	Date.	No. of Plates.	Star <i>a</i> .	Star <i>b</i> .
	June	52	"00	+ '05
	Aug.	28	- '07	- '02
	Sept.	52	- '01	- '06
	Oct.	20	- '01	+ '11
	Nov.	28	- '03	+ '01
	Dec.	32	+ '01	+ '06
1887.	Jan.	28	+ '10	+ '05
	Feb.	24	+ '11	- '15
	Mar.	16	+ '08	- '04
	April	32	- '10	- '08
	May	44	- '01	+ '05

This table indicates, with comparatively good agreement between the two independent determinations *a* and *b*, that the distance of the components of 61 Cygni was less in 1887 January than it had been in 1886 September by about 0''.12. Just such a diminution of the distance in four months was found by Dr. Wilsing to precede minima in 1891 June and 1893 April. Moreover, Professor Pritchard's results agree fairly well with Dr. Wilsing's in showing an indication of steadiness for some months preceding the minimum, and a sharp rise, with perhaps a subsequent slight fall, some three months or so after the minimum. If the minimum observed at Oxford is taken to have occurred in 1887 January, we can compare it with Dr. Wilsing's of 1893 April, to determine the period. The interval would give a period of twenty-five or nineteen months, according to whether we assume that three or four periods had elapsed. Dr. Wilsing makes the period twenty-two months from his own two observed minima. The period may therefore be variable.

On the whole we may conclude that the above evidence furnished by the Oxford observations is favourable to the reality of Dr. Wilsing's discovery. I have also examined in a similar way the Oxford measures of the comparison stars *c* and *d*, which are situated in a direction approximately at right angles to the great circle joining the two components of 61 Cygni. The numbers, however, come out smaller, and with more frequent changes of sign, so that no conclusion can properly be drawn from them.

Columbia College Observatory, New York:
1893, November 27.

The Orbit of γ Andromedæ BC. By S. W. Burnham, M.A.

For nearly forty years after the discovery of this pair by Otto Struve in 1842, the measures failed to show any considerable change in either angle or distance. The observations are very discordant, and for many years it was impossible to say anything more than that an increase in the distance of perhaps one or two tenths of a second was probable; while change in the angle was at least doubtful so far as the measures were concerned. Some ten years ago it was evident that the distance was decreasing, and it finally came to be regarded as single in most telescopes. I found it very close with 36-inch refractor at Mount Hamilton, but made some measures of both angle and distance in 1889, and again in 1891 obtained three fairly satisfactory measures of the angle, the distance being estimated at not more than $0''.05$. It was also examined in 1888 and 1890, but the star was too close, or the conditions too unfavourable, to make it possible to get any measures.

From the uncertain and discordant character of the earlier measures, it was impossible to tell to which quadrant the smaller component belonged in my measures of 1891, and therefore the form of the apparent orbit was still uncertain. During the past summer I requested Professor E. E. Barnard to observe this pair with the 36-inch telescope, and especially to be certain of the quadrant. I have recently received from him his measures, made on two nights under favourable conditions. On both occasions he was absolutely certain that the smaller component was on the following side. The stars were well separated with a power of 2,600, and the difference in magnitude was unmistakable. I have given the highest value to these measures.

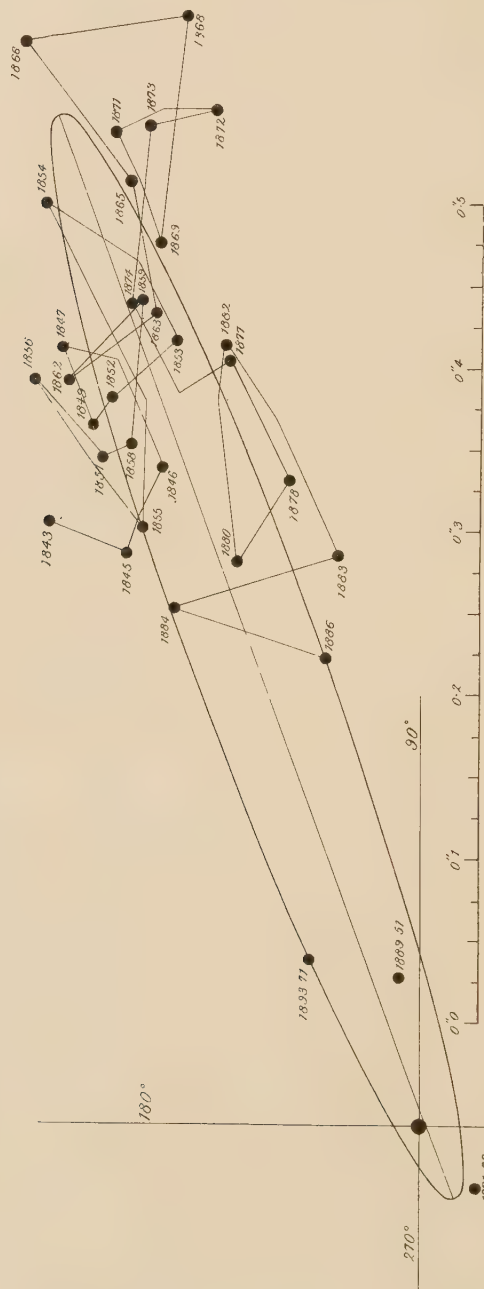
This position furnishes the key to the situation, and without it would be impossible to even guess at the form of the apparent orbit. With the large arc passed over by the smaller star, a reasonably good provisional orbit should now be obtained. According to the Mount Hamilton measures, this star has moved about 330° in the last four years.

The following are the measures of this pair :

Measures of γ Andromedæ BC. (02 38).

1843.00*	119.7	$0''.45 \pm$	2n	Dawes
.19*	119.8	0.35	2-1	Mädler
.55*	125.5	0.48	3	O. Struve
1845.15*	116.9	0.39	4	Mädler
1846.64*	111.3	0.43	7-3	Mitchel
1847.13*	117.9	0.52	5	O. Struve

1847·82*	111 ⁰ ·3	0 ["] ·6 ±	4 ⁿ	Dawes
1849·69*	114·9	0·47	4	O. Struve
1851·19	116·6	0·40	4	Mädler
1852·21*	114·5	0·48	2	Mädler
·78*	111·3	0·5 ±	2	Jacob
1853·23*	116·0	0·47	3	Mädler
·79*	108·5	0·55 ±	4	Dawes
·94*	106·8	0·4 ±	4	Jacob
1854·75*	112·0	0·61	1	Dawes
1855·02*	119·4	...	1	Mädler
·09*	109·8	0·40	1	Secchi
1856·12*	116·7	0·5 ±	1	Jacob
·20*	116·5	0·45	1	Mädler
·21*	121·7	0·41	2	Winnecke
·84*	113·0	0·67	3	O. Struve
·90*	109·7	0·47	3	Secchi
1857·23*	115·4	0·45	3-1	Mädler
1858·06*	114·0	...	2	Jacob
·22*	115·4	...	2	Mädler
·99*	108·9	0·45	3	Secchi
1859·81*	108·7	0·53	1	Dawes
1862·55*	115·2	0·50	4-2	Mädler
1863·27*	108·5	0·45 ±	8	Dembowski
·86*	107·7	0·59	1	Dawes
·99*	107·6	0·61	...	Romberg
1865·67*	107·1	0·59	4	Knott
·68*	106·9	0·60	1	Dawes
·76*	106·3	0·58	2-1	Talmage
1886·21*	110·0	0·70	3	O. Struve
·74	132·3	...	1	Winlock
·74	107·2	...	1	Searle
·74	100·4	...	1	Winlock
·85	104·2	0·64	1	Talmage
1867·79	104·3	0·5 ±	1	Newcomb
1868·82*	102·0	0·69	6·5	Brunnow
1869·84*	107·0	0·63	3	O. Struve
·95*	105·6	0·5 ±	13	Dembowski
1871·01*	110·6	0·63	15	Dunér



Orbit of γ Andromedae B.C.

1872·83*	101 [°] 5	0 ["] 63	4-2 ⁿ	Brunnow
·92	91·8	0·5 ±	2-1	Wilson & S.
1873·17*	105·4	0·63	5	O. Struve
1874·00*	109·3	0·53	1	Newcomb
·53	96·3	0·51	2	Gledhill
1876·79	105·7	...	1	Wilson & S.
1877·05*	104·1	0·48 ±	6	Schiaparelli
·71	103·9	...	1	Dobereck
·94	102·4	0·84	1	Seabroke
1878·21*	101·0	0·36	8	Hall
·65*	102·1	0·43	2	Burnham
1880·06*	107·9	0·36	1	Burnham
·11	106·7	...	2	Seabroke
·12	94·1	...	8	Jedrzejewicz
1882·05*	104·0	0·49	6-1	Bigourdan
1883·15*	93·1	0·29	7	Engelmann
·16	106·7	...	1	Seabroke
·87*	103·1	0·40	2	Perrotin
1884·18*	113·3	...	3	Seabroke
·65*	117·6	0·35	1	Perrotin
1886·83*	101·0	0·29	1	Newcomb
1889·51*	98·2	0·09	1	Burnham
1891·72*	312·6	0·05 ±	3	Burnham
1893·71*	123·4	0·12	2	Barnard

Mr. Gledhill, in his *Handbook of Double Stars*, gives two measures of this pair by Challis as follows:

1842·84	106 [°] 3	0 ["] 15
1843·31	115·8	0·31

I have not been able to refer to the original publication containing these observations, but as one of the angles must be erroneous, and the distances impossible, I have omitted them in the foregoing table.

The measures shown on the accompanying diagram (Plate 4) are marked with an asterisk (*), and the consecutive positions connected by lines to show the order of time in which the observations were made. When more than one observation in a year is used, a simple mean is taken. It will be seen at once that the measures as a whole are inconsistent and uncertain to an extraordinary degree. Evidently some of them are little, if any,

better than careful estimates. The distances in this instance seem to be almost as doubtful as the angles, which is very unusual in a pair of this kind. These large errors are all the more remarkable for the reason that from the beginning of the observations down to less than ten years ago this has been a comparatively easy pair, even with telescopes of very moderate aperture.

In finding the apparent orbit, it has been necessary to be governed largely by the more recent measures. After various trials, the ellipse shown on the diagram was adopted as the best that could be found to satisfy the law of areas in respect to the intervals between the late measures and the general group of earlier positions. It is satisfactory in this respect, and should give as good approximate elements as can be found from the present data.

From the apparent orbit we have the following elements :

$P = 54.8$ years	$i = 78^{\circ}.9$
$T = 1892.1$	$\Omega = 113^{\circ}.5$
$e = 0.875$	$\lambda = 200^{\circ}.8$
$a = 0''.37$	

Apparent Orbit.

Length of major axis	=	$0''.697$
Length of minor axis	=	$0''.077$
Angle of major axis	=	$110^{\circ}.0$
Angle of periastron	=	$289^{\circ}.3$
Star from centre	=	$0''.30$

According to this orbit, the distance should steadily increase, the position-angle changing very slowly from year to year. It will soon be readily measurable with ordinary refractors.

Chicago :

1893 November 15.

Windsor Measures of a Centauri in 1893. By John Tebbutt.

The following measures of a Centauri were taken with the 8-inch equatorial at the request of Professor T. J. J. See, of Chicago. As there will probably be no other double-star observations from this observatory for the current year, I deem it advisable to send them at once for publication in the *Monthly Notices*. In all the observations east of the meridian the line joining the observer's eyes was parallel to that joining the components, but in those west of the meridian these lines were at right angles. The scale of weights is from one to five, one denotes the worst, and five the best conditions possible.

Date of Obs.	Position Angle.	No. of Obs.	Distance.	No. of Obs.	Mag. Power.	Hour Angles between which the measures were made.		Weight 1-5.
1893.						h m	h m	
411	...	...	20'42	10	170	2 45 E	2 20 E	2
411	206'3	10	...	...	230	1 53 E	1 40 E	2
416	207'0	10	...	...	230	0 25 E	0 10 E	2
419	206'8	10	20'02	10	230	2 31 E	1 51 E	3
422	206'3	10	20'25	10	300	3 20 E	2 46 E	4
427	206'9	10	...	...	300	0 29 E	0 0	4
430	207'1	10	20'59	7	300	2 31 E	1 40 E	4
493	207'7	10	20'60	8	300	1 53 W	2 22 W	2
496	206'4	10	20'45	8	300	0 54 W	1 27 W	3
498	207'0	10	...	...	300	1 40 W	1 56 W	1
501	205'9	10	...	...	300	0 22 W	0 38 W	1

Weighting each result according to the conditions under which it was obtained, we have the following mean :

Epoch = 1893'442. Position angle = 206''8. Distance = 20''38.

*Private Observatory,
The Peninsula, Windsor, N.S. Wales :
1893 October 27.*

Observations of Double Stars made at Sydney Observatory.

(Communicated by H. C. Russell, B.A., F.R.S., Government Astronomer.)

The following list of double stars, believed to be new pairs, were found by Mr. R. P. Sellors between 1890 March and 1893 October, in the course of his double-star work with the 11½-inch refractor of Sydney Observatory.

No.	No. in Merid. Cat.	R.A. 1900. h m	S. Decl. 1900. ° ' "	Epoch.	P. Angle.	Dist.	Mags.
1	β Phœnicis St. 430	1 2	47 15	91'969 92'895 92'947 92'953	26°3 24'7 24'1 24'3	0'93 0'90 0'90 0'97	4-4
2	St. 448	1 4	47 12	91'969 92'895 92'953 92'956	210° ± 200°1 199°2 200°1	0'5 ± 1'09	7-9½
3	A G C 1690	1 39	55 22	92'977	179°3	1'0 ±	8-8½
4	A G C 3516	3 10	47 34	92'008 92'036 92'038	248°0 248°1 249°3	... 0'93 0'97	8-8
5	A G C 4117	3 39	48 33	92'036 92'038 92'049	187°7 188°4 188°4	1'81 1'67 1'81	7-12
6	St. 1898	4 23	53 20	92'038 92'055	98°8 98°0	0'88 0'74	7-9
7	Z Puppis St. 3044	6 27 ...	50 10 ...	93'088 93'104 93'162	262°9 265°4 267°2	... 0'68 0'68	6-6
8	St. 4451	8 29	52 52	92'266 92'276 92'320	303°1 298°9 303°5	0'5 ± 0'43	6½-7½
9	Z C 3683	8 46	63 27	93'156 93'194	2°4 2°7	1'02 0'91	8-9
10	A G C 16725	12 10	35 40	91'342 91'359	244° ± 244°1	1'5 ±	7½-8½
11	St. 8614	15 46	60 27	91'548 91'553 93'493	95°3 98°9 93°9	0'56 1'00	6½-8½
12	Z C 2139	16 33	47 51	93'586 93'608	178°1 178°2	1'02 0'98	8-8½
13	Z C 309	18 6	35 14	91'665	43°7	1'5 ±	9-10
14	St. 12307	23 45	52 16	91'934	70°4	0'90	7 7½

Observatory, Sydney :
1893 October 20.

*Observations of Brooks' Comet (c 1893), made at the Royal Observatory, Greenwich.**(Communicated by the Astronomer Royal.)*

The observations were made with the East, or Sheepshanks, equatorial, aperture 6·7 inches, by taking transits over two cross-wires at right angles to one another, and each inclined 45° to the parallel of declination.

The observations are corrected for refraction, but not for parallax. They are also corrected for the error of inclination of the wires and for the motion of the comet.

Greenwich Mean Solar Time.	Observer.	♂-★R.A.				Log Factor of Parallax.	Corr. for Refraction.	♂-★N.P.D.				Log Factor of Parallax.	Corr. for Refraction.	No. of Comps.	Appt. R.A. of ♄.		Appt. N.P.D. of ♄.		Comp. Star.			
		d	h	m	s			♂	♀	♂	♀				h	m	s	°		'	°	'
1893. Nov.	A.C.	9	15	35	20	+	5	49·87	0·00	-	1	43·8	0·7785	0·0	1	12	59	13·05	59	22	38·5	a
	"	9	15	35	20	+	4	52·17	0·00	+	0	38·7	0·7785	0·0	1	12	59	13·22	59	22	43·0	b
	B.	26	16	49	6	-	0	19·04	+0·01	-	7	53·4	0·4864	-0·1	3	...	...	...	...	...	...	c
Dec.	"	26	16	53	50	-	0	50·73	-0·01	+	7	16·2	0·4751	+0·1	2	...	...	...	...	...	...	d
	"	1	11	35	6	+	0	23·15	0·00	-	0	45·7	0·8918	-0·1	3	14	3	59·65	36	43	58·1	e
	A.C.	1	11	42	29	+	0	23·12	0·00	-	1	17·7	0·8872	-0·1	3	14	3	59·62	36	43	25·3	f
	B	2	14	59	47	+	5	12·09	0·00	-	2	17·3	0·6440	-0·1	2	...	...	...	...	...	...	f
	"	2	15	0	28	+	0	45·02	-0·01	+	9	14·7	0·6428	+0·1	3	14	9	13·26	35	25	14·0	g
	"	4	11	35	1	-	0	16·23	-0·02	-	10	55·0	0·8827	-1·4	3	14	18	28·47	33	16	54·7	h
	"	4	11	36	15	-	2	25·89	-0·01	-	5	43·5	0·8821	-0·8	4	14	18	29·42	33	16	53·4	i
	"	4	11	51	54	+	4	29·95	+0·01	+	10	21·6	0·8722	+1·2	2	...	...	...	...	...	...	k

Notes.

Nov. 9.—Distinct straight tail in south preceding direction.

Nov. 26.—Comet extremely faint; very diffused; sky bright; observations not very accurate.

Dec. 2.—Comet extremely faint; near a pair of faint stars.

Dec. 4.—Comet very faint; hardly any condensation; extremely difficult to observe; near a faint star.

The initials A.C. and B. are those of Mr. Crommelin and Mr. Bryant respectively.

Assumed Mean Places of the Comparison Stars.

	Comparison Star.	Assumed R.A., 1893 ^o .			Assumed N.P.D., 1893 ^o .			Authority.
		h	m	s	°	'	''	
<i>a</i>	W.B. (2) XII. 1025, 1027	12	53	21.96	59	24	9.6	Leiden Ast. Gesell. Zones, 43, 44.
<i>b</i>	W.B. (2) XII. 1041, 1042	12	54	19.84	59	21	41.8	" " "
<i>c</i>	B.D. + 47 ^o , 2095	13	45	3	42	17	7	Bonn Observations, vol. v.
<i>d</i>	Anonymous	13	45	36	42	2		Difference from Star <i>c</i> .
<i>e</i>	B.D. + 53 ^o , 1689	14	3	35.84	36	44	16.8	Cambridge, U.S., Ast. Gesell. Catalogue.
<i>f</i>	B.D. + 54 ^o , 1648	14	3	55	35	26	4	Bonn Observations, vol. v.
<i>g</i>	B.D. + 54 ^o , 1653	14	8	27.70	35	15	31.8	Cambridge, U.S., Ast. Gesell. Catalogue.
<i>h</i>	B.D. + 56 ^o , 1728	14	18	44.35	33	27	23.4	Helsingfors Gotha " " "
<i>i</i>	B.D. + 56 ^o , 1732	14	20	54.99	33	22	10.1	" " " "
<i>k</i>	B.D. + 57 ^o , 1500	14	14	0	33	7	6	Bonn Observations, vol. v.

Royal Observatory, Greenwich:
1893 December 7.

Erratum in Hansen's "Tables de la Lune." By A. M. W.
Downing, M.A., D.Sc.

On page 406, 1900, Arg. 19,

for 14.44231

read 14.44213.

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